ROUND TABLE SESSIONS

General Chairman - D. C. Costello

GENERAL CHAIRMAN: Good morning, gentlemen. We are starting our round-table discussions this morning to add a little flavor to the Conference. We have finished our formal presentations. We would like to announce that the sessions will be conducted in a very informal manner.

We have received a tremendous response to the notices sent to your offices asking for questions. We tried to group answers to the questions into certain subject areas; there are probably some that will not be answered.

Each of the round-table chairman has organized his own session and there may be no uniformity in the pattern in which each is conducted; in general, I believe they will be well organized.

We want to have all participants feel free to ask questions; break in at any time and keep lively discussions going throughout the day.

You might also think about that part of the program at the conclusion of the round-table discussion. We want to discuss where to hold the next Conference. You might like to suggest how we organize the next Conference. Any other suggestions you want to pass on to Mr. Belter and Dr. Silverman will be appreciated.

PANEL A - ROUND TABLE SESSION
Thursday Morning, 24 October 1963

IN-PLACE FILTER TESTING

PANEL CHAIRMAN: The panel members for Panel A, In-Place Filter Testing, are:

R. W. Schneider, ORNL, Chairman
E. C. Parrish, ORNL
J. A. Young, NRL
J. J. Croley, SRP
F. E. Adley, HAPO
J. W. Thomas, HASL

We have heard a lot about the ability of high efficiency filters to remove particulate matter efficiently and effectively in the micron and submicron range. Filter systems can be highly efficient if the systems are properly constructed and the filters are properly installed.

As users, we must remember that we often put our knees, fingers, toes, and maybe other items, through the filter media. If we do not go to this trouble, we install the filters improperly. Or, if we install the filters properly, we have another problem with continuous leakage through the filter housing itself. In other words, the proof of the pudding is to run an in-place filter efficiency test after the operational filters are in place.

All of the quality-control procedures from the start of manufacture of the high-efficiency filter through the Quality Assurance Stations are extremely important, but today most of us stop at this point; actually, we have stopped one step short of doing the job right, and that is the in-place test. We cannot be assured that we have an efficient system unless the system is tested in situ with the high-efficiency filters in place. We can take all the precautions that we wish, but if we do not determine the efficiency of the system, we do not know what it is.

I am not trying to say that the system must be as efficient as the individual filters, because in some cases we can tolerate less efficiency; but the fact is that if the systems are not tested, we do not know what we have.

For the record, so that you will have reference material, there are two reports which cover a good portion of in-place testing: NRL Report 5929, "Studies of Portable Air-Operated Aerosol Generators," by W. H. Echols and J. A. Young; and, ORNL Report 3442, "Tests of High-Efficiency Filters and Filter Installations at ORNL," by E. C. Parrish and R. W. Schneider. The Proceedings of the 7th Air Cleaning Conference, TID-7627, has several excellent references. The last is USAEC Report TID-7023, "Inspection, Storage, Handling and Installation of High-Efficiency Particulate Air Filter Units," by H. Gilbert and J. H. Palmer.

Before we ask for questions, I have requested each panel member to take a few minutes to discuss a phase of filter testing, in-place testing, or other topics relative to the title of this panel session.

NRL PANELIST: I want to take just a few minutes to mention two studies which have been accomplished in the last two years ar NRL. The first has already been mentioned, and that is the study of portable air-operated aerosol generators. These units have found wide use for in-place testing.

The other study is that which we have just completed; the report is not yet available. On the shock and vibration characteristics of high-efficiency filters, we had each of the three manufacturers of filters in this country prepare for us, under careful control using paper supplied by the Navy, a number of filters. Each of these filters were evaluated as to efficiency and resistance characteristics. These were then subjected to the Navy standard shock and vibration tests. The objective of this study was to determine if the filters, as they were being produced, and as they were being produced in the finest quality, could withstand standard Navy shock tests, which evaluate the proposed use in environment aboard ship.

Each of the filters successfully passed the vibration tests, a series of tests in which a filter is subjected to an increasing frequency in amplitude in a very regulated and standard pattern. The filters were evaluated as to efficiency and resistance before and after this series of vibration environmental tests, and then the filters were subjected to the shock tests. The filters were subjected to an increasing repetitive blow on the frame which houses the filters, not the filter itself, and it was found that all the filters passed the vibration environmental tests. None of the filters were able to pass the shock test. This report should be available in a matter of a few months for your evaluation.

Of course, since the 7th Conference we have been continuing our studies of the methods and techniques for in-place filter evaluations. I want to point out, however, that there is much more than merely measuring the filter efficiency. One must also be aware of other problems of air balancing, air-flow distribution, pressurization, and the whole endeavor which we prefer to call the system evaluation. The in-place testing is a part of this overall system evaluation.

ORNL PANELIST: We feel we have come a long way since the Seventh AEC Air-Cleaning Conference. At that time we had tested only one or two systems in situ, and today at ORNL we have tested about 2,000 systems in place.

The test of filter systems in situ is not difficult. The procedure used by ORNL consists in discharging an aerosol produced by atomizing liquid DOP with compressed air into a convenient air intake ahead of the filter bank. The concentration of the unfiltered smoke is measured by drawing a sample from the duct ahead of the filter bank and passing it through a forward light scattering photometer. The concentration of the filtered aerosol is then measured from a sample withdrawn downstream of the filter. The filtration efficiency of the system is calculated from the two concentration values. In general, the in-place test is conducted according to this procedure, but in a few instances it has been necessary to modify the basic techniques to circumvent physical limitations imposed by the particular installation.

Several slides will now be shown.* The first shows the removal of an upstream sample during an in-place test. The downstream sample will be withdrawn from the pump discharge, so that we will be sure to have a well-mixed, representative sample.

When an in-place test shows that the efficiency of the system is unsatisfactory, the source of the leakage can be found by probing the downstream side of the filter bank. Leaks are indicated by the erratic behavior of the needle on the amplifier when the probe picks up unfiltered aerosol particles.

The results of the first in situ tests of the 486 different systems will be shown by the next slide. It seems reasonable to expect that the filters serving chemical laboratory hood exhausts might have lower first-test efficiencies than systems handling air that was essentially free of chemical vapors, and the two categories were inspected accordingly. It appeared, on the average, that small systems containing one or two high-efficiency filters should give a higher first-test efficiency than systems with a greater number of filters, so Category 2 has been subdivided to show this trend. Only installations containing one or two filters are tabulated in Category 1, since very few of our systems serving laboratory hoods exclusively contain more than two filters. The term "first-test" is intended to mean the first in situ test of a particular system, and bears no relation to the age of the installation, or the length of time the filters were in service prior to the test.

On the basis of the tests, and experience in general, we can draw some conclusions. It is relatively easy to achieve acceptable system efficiencies in small single- and double-filter installations. No significant difference is apparent between the Categories 1 and 2. High efficiency is more difficult to achieve in the larger systems. Only 31% of the systems containing 3 filters or more ran an efficiency of 99.97% or better on the first test, as compared with about 76% for the smaller installations.

In a few instances low efficiencies were found to be the result of corrosive attack of the media. In one such case the system efficiency was 2%. In general, however, efficiencies less than 99.97% were attributed to faulty installation of the filters, damage to the filter media during installation, gasket leakage due to inadequate compression, and inadequacies in the filter housing and filter mounting frames.

New systems of all sizes have a reasonably good chance of passing a pre-operational in situ test if close attention is given to details of design and construction, and if the filters are properly installed. Two new systems of 30,000 and 80,000 cfm showed efficiencies of better than 99.97% on the first

* Illustrations not available

tests, and on two subsequent tests, without the need for remedial measures.

We found that most existing systems can be tested in situ, but the task is usually easier, and more economical, when new installations are designed for, and existing systems are modified to facilitate in-place testing. Our experience at ORNL indicates that in-place testing is a convenient and reliable tool for verifying the adequacy of systems and for locating sources of leakage, yet the cost represents a very small fraction of the cost of the building.

HASL PANELIST: I would like to make a few remarks on the significance of the pinhole effect, as far as in-place testing is concerned. One of the most important things that one should bear in mind is that filters may give a low efficiency at low flow rates than when tested at the rated flow. Perhaps some maintenance man would get the idea if you are operating below capacity, a filter may last longer and work better, but this is not necessarily true, and this should be looked out for.

Particle size is not so important in in-place testing as it is in evaluation of filter materials, which follows from the fact that if one had leaks, then the penetration is a strong function of particle size. To emphasize this point I would like to show one of my previous slides, which you have already seen, Number 6. You notice those two curves at the bottom are for two different aerosols with no pinholes, and the two curves at the top are for the two different aerosols with pinhole. If one curve is subtracted from the other, the small aerosol curve on the top with the pinhole, and the small aerosol curve on the bottom, effect of the pinhole can be determined. Doing the same thing for the bottom curve, the differences are going to be nearly the same; this is experimental evidence that it does not make too much difference on the particle size, as far as the penetration to a pinhole filter is concerned. This is important in in-place testing.

Some have raised the question that we test on 0.3 micron smoke from a thermal generator but use the air-operated generator on in-place testing. These results seem to indicate that use of air-operated generators is not as bad as some might think; we are happy to note this, because it would be next to impossible to try to use a thermal generator for in-place testing.

SRP PANELIST: Our containment vessels were shown yesterday. Two things, I think, stood out with our testing. One, we were very much surprised at the number of rejected filters we obtained. We just completed one round of reactor testing. Out of the first two banks in the first reactor we rejected approximately 32 filters, which was rather astounding. This continued through the third bank at the same ratio. We talked with our maintenance people, and things improved considerably.

The other thing I think interesting about it was the rate of pluggage of our filters. We have not installed the pre-filters in the vessels yet. In filters that have been in service for as much as eight to eighteen months we are getting ΔP's across the bank of up to 4 inches; the rated flow is around 0.9 to 1. So, we are running into some problems in that respect. Our manner of testing, of course, is the customary one. I don't think I need to go into any detail there.

HAPO PANELIST: Before I touch on nuclei counting, I would like to get into one other possible method of evaluating filters. I might mention that our particular group at Hanford is not in the routine in-place testing program; my remarks are more or less on some work we have done in our own interests and on a call-in basis for special, particular jobs.

I would like to touch on some of the HASL remarks about the pinholing effect. The first slide shows what we found with respect to single specimens on which we were able to run tests on air flow in the device, and, without removing it, adding a 1/8" hole to the paper. Curve A is essentially the filter in its original condition; curve B, after we added the 1/8" hole. We subtracted the two to obtain curve C. The calculation was simply an orifice calculation.

The next slide is the same thing on an expanded scale. We took a 1,000 cfm filter, and using the standard E-18 DOP Tester were able to show these effects. The bottom curve is essentially a good filter with a penetration rating of .01% at a rated airflow of 1,000 cfm. As the capacity drops, the penetration also reduces. After this, we added two 1/8" holes to this filter, consecutively. The first hole produced the middle pair of curves resulting in an inverse relationship between airflow and penetration. The first pair of curves in the center of the slide represents the holes in the upstream and downstream positions; the larger or higher penetration occurring when the hole was in the downstream position indicating, possibly, that the orifice flow on the discharge side was not interferred with by separators. The top pair of curves is essentially the same thing, with two 1/8" holes in the filter essentially in the same position.

The next slide was based on a similar type of test, with a selection from filters which we happened to have at the testing station. Those filters had roughly .01% penetration and showed an inverse relationship, similar to those experienced with the hole added to the filter.

The curves above .01% penetration essentially indicate that, probably in some cases and certainly in others, a defect in the one with the hole, while the other filters appear to be of good integrity.

To me, condensation nuclei counter is a rather new instrument. People have been working with the instrument or the principle for quite some years, but our engineering laboratory in Schenectady developed a prototype earlier this year which was made available. I was interested to see what this would do in the way of evaluating a filter's performance. I was largely interested because it is so sensitive that it will read from ten to several hundred thousand nuclei per cc.

The instrument weighs a little less than 200 pounds and is portable or mobile. I found that functionally it was a real good instrument.

We performed tests with 1,000 cfm filters and compared with them the DOP results, finding there was no good quantitative relationship, which was rather disappointing. More work is required to determine the relationship. Roughly, it appears that the penetration was about 0.1 of that found by DOP. We ran a series of filters and found out that the results varied roughly in that proportion.

In addition to the single filter test, we applied it to a bank of filters handling about 1,000 cfm. To deal with nuclei we fed a sodium chloride mist from the regular atomizing or aspirating type generators, and put the sodium chloride mist into the gas feed with an acetylene torch where it was essentially burned inside the acetylene flame; this is a simple and easy generator to devise and it worked very well.

In a system with 100,000 cfm capacity, we could maintain a nuclei count of about 1.25 million nuclei per cc on the upstream side.

We were able to run a series of eight tests on this particular bank, and we did not know the exact condition of the bank at the time, but the tests

ranged from 0.08% penetration to 0.2% penetration, with an average of about 0.12%. Applying 0.1 velocity relation between DOP and nuclei, I would say that this particular bank had a penetration of 0.12%. Whether this is right or not, I don't know, but based on Parrish's earlier presentation, 0.8% of his results fell within this group; it is reasonable to expect that this could have been operating somewhere in that range.

We have used uranine for special tests on custom made, high-efficiency filters at Hanford. We find uranine to be a good test method. It is simple; it is inexpensive, very sensitive, and testing can be accomplished with the class of instruments found usually in most laboratories.

PANEL CHAIRMAN: We are now going to declare the session open for informal discussion. Any question pertinent to the subject of in-place testing or the testing of individual filters is fair play. In addition, one panel member can question another panel member; or, if we get a question from the floor and I ask one of the panelists to answer, if any of the other panel members object or has a different idea or a different interpretation, by all means he is obligated to speak up.

QUESTION: When generating a heterogeneous aerosol, as with a portable NRL generator, what efficiency are you really measuring? Is this a count efficiency, or is this a mass efficiency, or surface area efficiency? If two experimenters use the generator, both with the same diameter, but one with a sigma of about 1.2 and one with a sigma of close to 3, will they get identical results if they test it on the same filter?

NRL PANELIST: First, what you are actually measuring during an in-place test are the deficiencies in the filter. If one takes a piece of filter paper with a holder that one knows has absolute integrity, and measures the difference in penetration between, say a 0.3 micron of a heterogeneous aerosol and one of these aerosols which has a light scattering mean diameter on the order of 0.8 or count mean diameter on the order of 0.3 or 0.4, one does obtain a difference in penetration. When this is done on a large scale with a filter with some 200 sq. ft. of total surface, then one finds that the penetrations may be of the same order of magnitude, and often are. This is not because of the difference in penetration through the paper, but because of the penetration through the holes or leaks. It is not dependent upon the size of the aerosol particle. Does this answer your question?

QUESTION: That answers the second part, but I just wonder what kind of efficiency we are measuring, whether it's just a relative number?

NRL PANELIST: You have a relative number. This depends upon the light scattering chamber. This is what you are using to detect the particle concentration.

ORNL PANELIST: We have a number of different generators of the same construction at ORNL and we use them interchangeably and we can duplicate our results very well.

QUESTION: I would like to find out the recommended treatments for in-place testing of filters.

ORNL PANELIST: The frequency with which filters need to be tested is something that everybody will have to settle for individually. We have systems that are on a frequency of once every six months. We have others that are on a frequency of once every three months, and we occasionally have systems that are tested once a month, and we've had occasions with systems that were tested

once a week.

QUESTION: Has the nuclei counter test been used on large filter banks at HAPO?

HAPO PANELIST: No, except for this one test. Our results are limited. Naturally, we had the instrument on a short-term loan and did limited testing. We only used it on one bank which had been in for some years actually.

QUESTION: On the NRL vibration testing was the efficiency of the filter tested, or was this just a physical test?

NRL PANELIST: The efficiency of the filter was determined after each of the vibration studies; efficiency of resistance was determined before and after the vibration, and then before and after the shock studies.

ORNL COMMENT: With regard to the previous comments about efficiency, my opinion is that if penetration through filters is released, then you are measuring the number or per cent that gets through. Actually, if the penetration is 1%, then 1% of the aerosol particles get through if it is due to leaks through pinholes. But if the particles are going through a filter with selected removal of certain sizes, then all you are measuring is a light scattering of the aerosol coming out, and it does not represent a change in number concentrations.

HACL COMMENT: I would like to throw a little controversy into this discussion because I think the question of whether you should use a liquid or a solid aerosol doesn't seem to have been identified here as a problem, and I think the vibration test question which preceded would indicate that if you tested a DOP, which is a liquid aerosol and it soaks into the fibers, you couldn't see any effect of vibration on release of deposited material. I would for that reason, if for no other, lean towards a solid aerosol test, and I would like to hear some discussion on this subject.

NRL PANELIST: The shock and vibration tests were not designed to study fundamentally the effect on filtration, but the effect to the particle. We wanted to know if these filters could withstand the environment to which they would be subjected. We feel that a much more fundamental study is required to answer the latter question, and we are sure there would be a difference between liquid and solid aerosols. We used DOP simply because it was quick, easy, and convenient, and not for any other reason.

PANEL CHAIRMAN: I would like to add to that point. From the ORNL standpoint, we were faced with a problem of testing systems in-place, and these ranged anywhere from 25 cfm up to 200,000 cfm. We needed a method of generating enough smoke in order to get a reasonable upstream concentration, and this is one reason we went to the liquid aerosol or DOP. We have run thousands of filters through the rig, and we were testing the individual filter and not looking for leakage in the system. After running several thousand, we found that at rated flow we correlated with the Quality Assurance Stations at Edgewood and now at K-25. The correlation was excellent. So we felt that any number that we got was comparable to any number the Quality Assurance Station got. We were not particularly interested in fundamentals of particle physics, or anything else. We wanted to be able to duplicate the Assurance Station results so that our numbers would have the same meaning.

QUESTION: I would like to ask a question on the status of the filter problem and its testing with respect to national standards. Are there standards for testing and are there standards for the installation of filters?

PANEL CHAIRMAN: I know of no standard. Of course, for the testing of new filters by the Quality Assurance Station, there is a military standard. It is an excellent question and I suggest that it be brought up again.

QUESTION: In connection with use of an absolute filter what efficiency is the minimal acceptable? Would you say that a penetration of .01%, more or less, is satisfactory?

ORNL PANELIST: We cannot tell any operation how efficient to be. Our radiation safety and control people have a release which requires filter systems to be 99.95% efficient, or better, unless they can show cause why they should accept efficiencies that are lower than that.

HAPO PANELIST: We have at Hanford the equivalent of the Oak Ridge testing facility, and at times have filters coming through from the different sites, where the filters are going to be run at considerably different flow or capacities than the rated capacity.

You will recall the curves that were shown earlier; if a filter has a pinhole and is satisfactory at the rated airflow, and then operated at a different airflow, there could be penetration which is considerably different, or non-acceptable according to the AEC standards.

PANEL CHAIRMAN: Some of the systems at ORNL under construction will be handling contaminants. Filters for these have come from the Assurance Station for testing at the rated flow. We are checking those filters through at 100% of rated flow, and at 20% of rated flow looking for pinholes. We are finding a few. These are being sorted out and not being used for the particular facility.

A pinhole test was run yesterday or the day before on our rig and we found a particular filter that ran 99.962% efficiency at rated flow, and it was down to 99.62% at one-fifth rated flow, a significant drop. Of course, in this particular case the pinholes were artifically introduced, but this really makes no difference.

COMMENT: First, I would like to say how much I agree with Mr. Thomas on the fact that pinholes are not highly selective; and, as the velocity goes up, the penetration certainly goes down.

Secondly, we have found at CDEE after having put artificial pinholes in corners of filters, that the particulately laden air from these slow leaks have followed the flow line a long distance down the ducting. Depending upon where you sample, you can get factors of penetration varying by a fraction of 0.25. I would also like to know what mixing has been carried out in the ducting after the filter?

NRL PANELIST: We recognize this problem and attempt at all times to make sure that we have representative samples upstream and downstream. Depending on where the blower is we have found that the best way to get a good sample is to take a sample after the aerosol passes through the blower. The blower has low removal efficiency, but it does an excellent job in mixing. Another technique which we often use in our system, which is smaller than at ORNL, is to use manifold sampling techniques; but we are constantly aware of this difficulty.

NRL PANELIST: Have you done any filter evaluation with your salt technique, either with a portable rig or at another installation other than in a laboratory?

CDEE COMMENT: No. All we have done is to test the filters in the laboratory, and we have never done any <u>in situ</u> testing on big installations. We have

tested 1,000 cfm filters. We have also tested for leaks in the filter. What we haven't done is to test one of your 20,000 cfm units.

SRP PANELIST: I understand that ORNL has been testing large banks in situ, whereas SRP has only tested individual filters in each of these banks. Now how do you obtain representative sampling on a bank of 100 filters at 100,000 cfm?

ORNL PANELIST: Whenever we can, we go downstream of the blower. If we cannot do that, then we try to take enough samples at whatever point is available to assure that we are not missing leakage because of transportation or channeling.

PANEL CHAIRMAN: We do put in one other thing. When the probe is shoved into the duct, we move the probe around to try to cover a number of points in the duct and look for any erratic behavior of the amplifier. If the needle jumps, that is an indication that there is leakage and that you must look closer.

COMMENT: I was interested in what was said about use of nuclei counters in testing filters. This surely must be a function of the particle size of the aerosol you are using. Have you ever used a micron DOP smoke? I think you are still working on this. It certainly works on things wettable, such as polystyrene. If it does work with DOP, I think the results must be the same.

HAPO PANELIST: They may or may not be the same, but I agree with you 100% that more work is needed on this particular point. If it has not been explored, it should be.

The way we performed this particular test was to feed in the aerosol a considerable distance upstream from the filter bank, probably 100 feet. Assuming we obtained good mixing by the time the leak in our contamination concentration arrived at the filter bank, we sampled about 100 feet or so downstream from the bank. There is a relationship, I am sure, between the nuclei size, which can vary upwards from 0.001 microns to some substantially larger figure. I am sure it has some effect on penetration.

HAPO PANELIST: Were the Navy filters tested standard filters with rubber cement, or were they the glass-pack type?

NRL PANELIST: We actually did both to evaluate the standard construction, and to a degree the effect of the glass packing. In both cases, vibration was not the problem. The shock treatment was severe; in fact, it tended to blow the filter elements right out of the case. I was quite interested in the way the filters actually were damaged. I think it will be our conclusion that the only answer to making a filter of the type acceptable for the environments which we are considering would be to decrease the span; that is, to utilize perhaps a 1 x 1 filter and to improve the shock isolation techniques of installing the filter in the housing.

QUESTION: In what direction was the shock compared to the orientation of the filter?

NRL PANELIST: Actually it was at an askew angle, so that the filter was held in a position to achieve a certain intensity of blow calculated from each of the three directions following standard methods that have been developed over the last twenty-five years.

HAPO PANELIST: Did you find any appreciable difference or increase in the deterioration of the middle frame?

NRL PANELIST: No. Actually the range in which the filters failed were for

approximately the same severity of blow for all types of filters evaluated.

COMMENT: I am particularly interested in any techniques for large bank DOP testing or solid particle testing of particular filters when activated carbon beds are installed downstream from such filters. What is the method of downstream sampling? Should it be upstream or downstream of the carbon filters? With regard to Dr. Silverman's point, when a liquid aerosol may be used, does anyone recognize the possibility that penetration of a liquid aerosol through the particular filter may be trapped on the carbon filter, and thus not give an indication of penetration by sampling made downstream of the carbon filter bank?

HASL PANELIST: I would like to make a few remarks on this question of liquid versus solid aerosols for testing. It is my belief that if you run short tests on the order of, say one-half to two minutes, and you have small particles of under 1 micron, it won't make the slightest bit of difference in the penetration values you get, assuming, of course, that the particles have the same density. It seems to be just a matter of which is the most convenient and easiest test to use; there shouldn't be any difference in the penetration. I cannot see any theoretical reason why there would be the slightest difference in penetration of a liquid and solid aerosol of the same density, provided the particles are small enough not to blow off the filter. So I think it's a matter of convenience.

Carbon beds are very inefficient on particles, particularly the larger ones. There will probably be some removal by the carbon. It would probably be best to sample upstream from the carbon, but I don't think there would be a big difference.

HAPO PANELIST: We ran some of the nuclei counting tests on filter bank evaluation. Our particular case was a bank of about 100,000 cfm filters backed up by the charcoal bank a few feet downstream, which would make detecting penetration particularly difficult if the samples were taken between the banks. We went downstream about 100 feet, but we have to go about the final analysis in a round-about way. We were able to determine what the holdup was in the charcoal bed to make a correction for that. In this particular case we had a rather common commercial charcoal filter in use, and estimated our particle size about .007 microns. With that particular condition we had a penetration or an efficiency of about 24% or 25% in the charcoal bed. We just assumed this as a correction factor and determined what the absolute filter bank was doing from that. It is not accurate, but it was the best we could do.

SRP PANELIST: I would think that with a well-installed absolute filter, a rather high-efficiency bed, that the penetration of the DOP into the charcoal would be relatively minor.

ORNL PANELIST: This, I think, is true. We had several systems that had charcoal either following or ahead of the filters, and we found that it makes no difference. As a matter of fact, on a small system we ran a duct test, and without our knowledge the charcoal filter had been substituted for the particulate filter and we were surprised to find the efficiency at zero.

HASL PANELIST: I would like to make a few remarks about deficiency of carbon beds for taking out particles. Everybody knows that if the particles are the size of gas molecules, there is essentially 100% efficiency. That is because of the high diffusion rate of gas molecules; this is a mechanism that gets particles over to the surface of the carbon where they can be absorbed. So we can expect small sized particles, such as .007 micron, are going to diffuse and we can expect some deficiency, probably something like this. If we use .007 micron, we might get 25% removed. Now if we use .07, a few per cent

removed; and, larger than that practically nothing until the particles are so big they begin to impact on the carbon. Then the carbon acts like a filter. For very small particles in the Angstrom Unit, we can expect efficiency from a carbon bed, but certainly not for particles as large as DOP. If I wanted to worry about this, I would worry about the DOP vapor using up some of the sites on the carbon.

ORNL PANELIST: Another thing that we need to consider is sampling location in an in-place DOP test, and what kind of roughing filters are installed. Frequently we find that the roughing filters are efficient enough to cause an appreciable drop in concentration before the aerosol gets to the high-efficiency particulate filters.

COMMENT: I would like to ask for some definitions of pinholes. Mr. Thomas' paper yesterday described a leak of 1 cfm at 1,000 cfm for total filter capacity. In doing a little simple arithmetic and accepting his premise that this was turbulent flow through the hole, using a Reynolds Number of 5,000, it turns out that this pinhole is about $\frac{1}{4}$" in diameter. What kind of pins were you using?

HASL PANELIST: That was a hypothetical example; the pressure drop across the pinhole is a square root function and the rest was lineal. The biggest pinholes we made were about $\frac{1}{2}$ mm in diameter. That does not represent any experimental results; I hope we got the point across, but that is all we intended to do.

COMMENT: This is my point exactly. I think your analysis is not completely accurate on the basis of your premise, because if we had two pinholes perhaps a few thousandths of an inch in diameter, and we have the equivalent of a $\frac{1}{4}$" hole, we would end up with about 30,000 pinholes, or something on that order. At that point, we no longer can be sure that we have turbulent flow. We will have, in effect, capillary openings and we would expect to get streamline flow through such openings, just as we do through the paper. And if this is so, I would then expect it to be 1:1 with the filter material.

HASL COMMENT: I don't agree with you. Maybe I don't know my fluid mechanics; but, if we have a capillary tube, we will have pressure drop through the body of the capillary and pressure drop at the ends. But whether we have turbulent flow or not, we still have the square root relationship at the end of a capillary tube. It doesn't matter whether it's streamline or turbulent flow, the pressure drop is the square root of function.

COMMENT: If you are thinking of the streamline flow as being the Reynolds Number around the individual fiber, as you do in a very porous bed, when you get into porosities of 98% or 99%, this gives you quite a different situation. Some analyses of work on fibers with cross-flow seem to confirm the fact that with a fairly dense packing of 20% or 30%, we get truly streamline flow through the filter, and it behaves as a series of parallels, pipes of capillaries.

HASL COMMENT: I will agree that if a pinhole is small enough it would not act like the pinholes I am describing. I guess I'll have to say that my paper applies to pinholes that are not of microscopic size.

PANEL CHAIRMAN: On this question, we have put pinholes in our filters; and, believe it or not, we used pins to put them in. So I guess we can call them pinholes. But this is neither here nor there. We ran the particulate filter over a range of flow, took the efficiency versus flow, and it does fit what you would expect with laminar flow to the paper and turbulent flow through the hole.

Whether this was a coincidence or not, I won't say; but the data consistently repeats itself.

COMMENT: I don't doubt the observations which you and many other people have made. This is not the issue. It is the explanation for the phenomenon that I'm calling for. One reference Mr. Thomas did not quote in his paper was some work that was done by Smith and his coworkers at A. B. Little several years ago, in which bond paper was used; they were quite surprised to find that a pinhole through a piece of impervious paper would take out something like 35% of the atmospheric dust. This is a true removal phenomenon, and, because of the flow rates they were using, it generated a great deal of turbulence at the entrance to the pinhole. This turbulence, in effect, centrifuged the particles to the outside of the hole, and after some time, a ring of dirt could be observed around the pinhole. This suggests a different type of action than the one we have been talking about.

PANEL CHAIRMAN: Before we continue with the questions, there are a few points that haven't been brought up that I think are important. I would like to ask a series of four questions just to bring out these points. The questions I have are these:

1. Is in-place testing difficult?

2. How much does it cost to get into the business?

3. How many in-place tests are conducted a year at ORNL?

4. How long does it take to learn the technique so that it can be applied effectively and efficiently?

ORNL PANELIST: In answer to your first question, in-place testing is not difficult; it is a very simple procedure.

To answer the fourth question, I would say that with one or two days of practice someone would be competent to run in-place tests employing the technique used at ORNL.

To answer the second question, I think the necessary instrumentation and generating equipment could be obtained between $1,000 and $2,500, depending on the size and number of installations that had to be tested. The difference in cost, of course, is in the generating equipment.

At ORNL the number of in-place tests conducted in a year would be approximately 1,000; probably a few more. We have made a few improvements in the last few months that will enable us to test faster. Ordinarily, four people comprise the in-place testing crews.

SRP PANELIST: The difficulty with the testing depends on where you are testing. If you are 50 feet up on top of a building, in a 40-ft. aluminum housing with 4-ft. innerspacings, it can be a problem; and, if you are using building air to pull through the filters, don't do it in February.

ORNL PANELIST: Perhaps I misunderstood the question, but the difficulty was with the technique. You are absolutely right about the difficulty of testing certain systems. Sometimes it is tremendously difficult. Large temporary ducts may have to be constructed in order to introduce aerosol in order to get well-mixed representative samples upstream of the filters; occasionally it is necessary to add to the downstream duct in order to get a representative downstream sample. The availability of compressed air in sufficient quantities is another

problem. Occasionally, we need to employ a construction type compressor with high output capacity and use the entire output on the larger systems.

QUESTION: Is the equipment for conducting these tests commercially available?

ORNL PANELIST: The equipment is commercially available. The instruments we use were developed by the Naval Research Laboratory. They are being manufactured by more than one manufacturer. Generating equipment is available, too. I don't believe there were any generators commercially available at the time we needed ours, so we built our own, using recommendations of NRL.

QUESTION: What per cent of flow is needed to get an optimum DOP test; that is, how slow can you go and not get into problems of pinhole effect, et cetera?

PANEL CHAIRMAN: The pinhole effect probably must always be present to some extent, and in many cases there is an advantage in testing a system at less than rated capacity because it does magnify the pinhole effect. On the other hand, I would not make it a general rule that it is desirable to test all systems at less than rated flow, because I can think of filters mounted on the downstream side of the filter frame, so that as you increase the flow, there is an unseating tendency which might produce gasket leakage. Therefore, my recommendation would be to test the rated flow to take care of the possibility of unseating the gaskets if they are mounted so they can be unseated, and also test at less than rated flow. I think this would take care of both complications.

QUESTION: Has anyone made an attempt to determine where and why these filters fail, and has any attempt ever been made to repair them?

NRL PANELIST: The answer is yes to both questions. What we have done many times is to tear the filters down. After looking for the source of the leak, we can dismantle a filter and find out the source; usually it's a hole. It is also true that we have repaired filters many times where the leak is obviously around the seal or where the end flap may have slipped. We have actually repaired filters; we don't recommend it as a matter of course.

SRP PANELIST: We find a lot of defective filters in our systems, primarily due to installation damage. If they were not too hot, radioactively speaking, we let them cool a bit and sent them back to the manufacturer. We did successfully repair these, by the way, in the past, and we then put them back in service at a very nominal cost.

PANEL CHAIRMAN: On Tour B and Tour E, you will see the filter testing equipment in operation. As a part of the tour we plan to conduct an actual in-place test on a small system. What will be shown in that small in-place test is representative of tests on systems up to 100,000 to 200,000 cfm.

If you have high-efficiency filters and if you need high-efficiency filters, you need the in situ tests.

In conclusion, we have considered all the questions that were submitted; they have either been answered by a panel question, or they were answered by the questions coming from the floor. I will also be free for most of the rest of this day, and tomorrow at the in-place test and the test at the rig. If you would like to see something that we haven't planned please ask, and we will try to demonstrate.

I would like to thank the panelists for their participation.

PANEL B - ROUND TABLE SESSION
Thursday Morning, 24 October 1963

SPECIFICATIONS, MAINTENANCE, AND MONITORING OF FILTERS

PANEL CHAIRMAN: We feel that Panel B, Specifications, Maintenance and Monitoring of Filters, will give you as much activity as did the last one, and that the information we bring you, of course, will be within the topic shown on the program.

Members of the panel are:

Humphrey Gilbert, USAEC, Chairman
Leonard Horn, UL
R. Mitchell, BMI
J. L. Murrow, LRL (Livermore)
S. E. Smith, UKAEA

Mr. Smith has already talked to you. He needs no further introduction. However, probably this should be supplemented a little bit. Some time ago, I contacted two friends in Great Britain and said, "You know my interest in the filtration business. For the 8th Air Cleaning meeting whom do you recommend that I contact in UKAEA?" And both of them, unhesitatingly, said, "S. E. Smith, of Aldermaston."

Mr. Smith, in addition to being senior member of the staff of the Atomic Weapons Research Establishment at Aldermaston, has been on the BSI Committee for Standardization, or I might say, for Re-Standardization, of the methylene-blue test, and he is also on the Committee for the Sodium Chloride test, with Mr. Dorman. Mr. Smith, as a panel member, will assure that we have the viewpoint of our fellow-workers on the other side of the Atlantic, to see if we really have advanced as we think we have.

Mr. J. L. Murrow is an industrial hygiene engineer with the Lawrence Radiation Laboratory. He has been engaged in the fire penetration study of filters, and while at Berkeley, was instrumental in developing a criticality-safe local exhaust system for machining fissile material. He made his fire-penetration studies in June of this year and this will be the topic of his discussion.

Mr. Leonard Horn is Associate Managing Engineer of the Electrical Department of Underwriters' Laboratory. Some few years ago he went to the reactor school at Argonne National Laboratory due, primarily, to his interest in atomic energy. He is quite a frequent face around the Atomic Energy Commission in Washington, and whether he has the title officially, or unofficially, he is looked at as a liaison engineer with Underwriters. At least all the AEC people,

when they have fire-protection problems, immediately search out Mr. Horn. Mr. Horn, of course, has been busily engaged in developing a Standard for fire tests of filters covering routine surveillance of manufactured items, which is consistent with the little, familiar UL label I am sure you have seen.

Mr. Ralph Mitchell is from Battelle Memorial Institute, where he has been located about 12 years in the Environmental Mechanics Research Group. He has investigated filter ventilation for one of the larger filter companies in the country, and in addition, he has investigated cigarette filter design. Consistent with this, he has also done lung retention studies for the tobacco industry.

Mr. Mitchell has been the chief investigator on a year-long project to develop a small-system, qualitative, in-place test, and we would like Mr. Mitchell to tell you about that now.

BMI PANELIST: I will try to describe the program which has been going on at Battelle in developing more or less an economical system for in-place filter testing of small filters and some of the strange findings that have evolved out of this program.

The system we are using, more or less, consists of a conventional aerosol package. We have a highly-fluorescent, oil-soluble tracer dissolved in Freon, and most of our tests have been with Freon-12 to obtain high pressure. We varied the particle size of the aerosol, which was produced by varying the concentration of the dye in the package, the pressure of the propellent, and essentially orifice diameter.

We evaluated a few materials as far as compatibility, plugging of the orifice, and what not, and we overcame these difficulties; we looked into the toxicology of the situation, and finally more or less settled on a package which we thought was suitable for filter evaluation.

We began penetration tests with filters which were supplied to us, and found in the initial tests we could get reproducibility. The penetration values obtained were quite a bit less than what we might expect. The filter was much more efficient with these materials. To check this, we reviewed the particle size of the solid aerosol particles. Now these particles which we produced are solid dye particles, completely solid, because essentially the only solvent we have is the Freon-12, and maybe a little co-solvent to prevent flashing at the orifice. So we end up with a solid dry aerosol particle, which is non-hydroscopic. We generated some aerosols, dibutylphthalate, and found particularly a quarter higher magnitude of penetration with essentially the same filters.

As we began running more tests on these filters, we had a fairly limited number of filters to evaluate, and there was no real good source of getting a large number of filters. We found as we ran our tests our penetration values went up. This is the same thing that Mr. Modrow was reporting on yesterday. This concerned us quite a bit, because we knew essentially our source strength was disseminating an aerosol of a known concentration, and we kept picking up more material on the downstream side of our filter. We are evaluating our penetration by generating a known concentration of aerosol, sampling upstream, though we didn't have to, because we always put up a known amount of the tracer. We sampled the downstream side of the filter and most of our tests, what we are using at the present time, is a high-volume sampler, with the Hurlbut all-glass filter paper in it, which produces the volume sampling rate of the high-volume sampler at about 14-$\frac{1}{2}$ cubic feet per minute.

When we began the investigation we thought maybe the material was coming off the duct itself, because in other programs we had run other tracer studies in this duct. We coated our duct walls with plastic materials. It didn't help the situation any.

Then we looked at the filter itself, and it was getting pretty hot on the downstream side of it. After that we thought maybe the material itself, the dye, might be causing some chemical reaction with the filter material, so we sent some of these filters to Oak Ridge to have them evaluated. They checked on DOP, and then we began a study to find out what was causing this phenomena, and found we were getting unloading with this load of about 2-½ grams of the dye deposited on a standard 1,000 cfm filter.

The first slide was made with UV light, and it shows a deposition on the upstream side. In this case we cut a section out of the filter and opened it up at the middle, at one of the folds. The absence of color locates the separators in back of the filter. The yellow is the fluorescent dye which we were generating. The second slide is the reverse side of this filter showing that the yellow dye is migrating through the filter media. In looking at this filter we saw little hot spots, and tried to make a filter media concentration profile, cutting it with microtome. We found that all the mounting media itself would dissolve the filter, and therefore could not get a concentration profile; we were hoping to be able to get a color picture.

The third slide shows a cross-section of the filter paper at one of these hot spots. This essentially indicates the variation in thickness of the filter media itself, and we got quite a step function. We have another slide of another hot spot showing just a regular crater effect. The filter thickness at this hot spot is roughly a factor of two difference.

This is as far as we have gone with this program in trying to find out why the filters are unloading. At the present time, we are running some tests with boron oxide fumes. We have collected the material downstream, but we haven't analyzed it as yet.

As far as our in-place filter testing, we have incorporated some dibutylphthalate in our package, so we are ending up essentially with a slurry, or you might say, with a particle which is liquid; it is essentially a slurry. In effect we have the dye, which is not soluble, and the dibutylphthalate in that it is in suspension, and we have run quite a few preliminary tests with this, showing we can get very good, high penetration values much different than if we didn't have the dibutylphthalate.

UL PANELIST: In 1958, Mr. Humphrey Gilbert of the U. S. Atomic Energy Commission Industrial Safety and Fire Protection Branch requested that Underwriters' Laboratories, Inc. give consideration to the development of requirements covering the testing and listing of high efficiency air filter units commonly employed in the ventilation and air filtering systems of atomic energy installations. At that time several of the component materials used in the construction of filter units were quite combustible and serious fires had occurred in filter banks. The development of units with better resistance to heated air and fire seemed highly desirable and Mr. Gilbert suggested that the experience and "know-how" of the Laboratories in the general field of fire protection would be useful in carrying out such a project. In addition, considerable trouble was being experienced with regard to perforation of the filter media prior to installation of the filter unit in the protected premise and it was felt that the Laboratories field inspection staff could make a real contribution to the quality of these units as they are shipped from their manufacturer's plants. We agreed to undertake this work and since 1958, we have been struggling

with the problem of establishing standard test methods which would assure improved resistance to the effects of fire in ventilating systems. Mr. Gilbert has suggested that the status of our work be reported to you today.

Following meetings with filter unit manufacturers, personnel from the AEC, the Edgewood Arsenal and the Naval Research Laboratory a First Edition Standard UL 586 was released dated September, 1959. The subsequent testing of commercial production samples of filter units in general use soon indicated that the performance levels which had been recommended by the Advisory Group were unduly high and apparently could not be attained by the designs and constructions of units then in production. Since that time much experimental test work has been carried out and one revision of our Standard dated October, 1962 has been circulated for review. Comments received, plus further testing with improved apparatus, has resulted in a second revision proposal which is now in the mail. A number of tests upon several samples of filter units in current production gives us a fair degree of assurance that units being currently supplied to your facilities will meet these latest requirements which in our opinion establish a reasonable safety performance level for units subjected to occasional or accidental operation under conditions of fire, heated air, moisture, cold and loading. It should be emphasized that this Standard contains a minimum of construction specifications and comprises, in the main, a series of tests and desired results to establish reasonable performance under the conditions which have been mentioned. Thus, any construction which meets the desired test performance stated in the Standard is acceptable and is eligible for the Laboratories' Label. Also, these requirements are minimum requirements, since the above-Standard performance of any construction is certainly desirable and is likewise eligible for labeling.

I have neither the time nor the desire to bore you with a long discussion of the details of our Standard, the developmental history of each test, etc. Instead, I will merely mention that our current proposal makes use of the following five tests - Heated Air Test, Moist Air Test, Low Temperature Test, Spot Flame Test, and Loading Test. The common feature of most of these tests is the measurement of filter unit penetration before and after each test, employing a dioctyl pthalate (DOP) generator, a forward light scattering photometer and electronic percent penetration indicator identical to the portable units which are currently being employed at AEC installations for in-place filter system testing. Limits are specified for percent of DOP penetration after each test.

To save time, let us proceed directly to a series of slides which illustrate the test equipment. Test details will be given with each slide.

Slide No. 1 shows an over-all view of the test duct employed for the DOP Penetration, Heated Air, Spot Flame, and Loading Tests. The duct is approximately 27 by 27 in. in cross section and 15-$\frac{1}{2}$ ft long, with a constant-speed blower supplying air to the upstream end of the duct. The air supply is metered by a venturi-flow meter and the blower is provided with an adjustable air intake opening.

Slide No. 2 is an over-all view of the upstream test apparatus, showing the percent of DOP indicator equipment, DOP generator, the venturi-flow meter, and the blower.

Slide No. 3 shows the interior of the test duct set up for the Heated Air Test, with the filter unit and the flame arrester removed. Air in the upstream section of the duct is heated rapidly by several gas flames and the duct is provided with a transite barrier to provide reasonably uniform temperatures across the filter unit face. The test consists of 5 min operation with the air heated to 700 plus or minus 50 F. Our current Standard proposal

limits percent of DOP penetration to 3 percent following this test.

Slide Nos. 4, 5, and 6 are photographs taken during Heated Air Tests. Incidentally, Mr. Gilbert is not an employee of Underwriters' Laboratories, Inc.

Slide No. 7 illustrates the arrangement used in conducting a Spot Flame Test. This test attempts to simulate the impingement of burning particles, such as small pieces of flaming pyrophoric metal, upon the upstream face of the filter unit. The flame is produced by a Bunsen burner adjusted to a blue cone tip temperature of 1750 plus or minus 50 F. The flame is applied for 5 min with the blue cone touching the surface of the filter unit. One test is conducted with the flame applied to a top corner of the filter unit in such a manner that the tip of the blue cone contacts the frame, filter pack and sealing materials. The test is also conducted at three points across the filter face. Inasmuch as the temperatures are sufficient to melt fiberglas filter media, no DOP penetration test requirement is applied following the Spot Flame Test, the sole criteria being the requirement that after removal of the test flame, there shall be no sustained flaming on the downstream face of the filter unit.

No photographs are available of the Moist Air, Low Temperature, or Loading Tests. The Moist Air Test consists of subjecting a filter unit sample for a period of 24 hr to a static atmosphere having a relative humidity of 90 plus or minus 5 percent at a normal room temperature of 77 F. In the Low Temperature Test a sample is placed in a static atmosphere at 27 plus or minus 3 F for a period of 24 hr and in each of these tests it is required that there be no change in the percent of DOP penetration measurements made before and after the Moist Air and Low Temperature exposures.

In the Loading Test, extreme loading of the filter unit is simulated by completely covering the upstream face of a test sample with a single layer of heavy kraft paper. The unit is then placed in the test duct and the blower equipment adjusted to provide a pressure differential of 10 in. water column across the filter unit. This differential is maintained for 5 min. As in the preceding two tests, it is proposed that there be no change in percent DOP penetration measurements made on the test sample before and after the Loading Test.

The final series of slides are photographs of samples subjected to the tests which have been described.

Slide No. 8 is a photograph of the upstream face of a sample subjected to the Heated Air Test. Its percent of DOP penetration before test was measured to be 0.008 and its penetration after test was measured to be 0.042.

Slide No. 9 is a second sample subjected to this same test. Its percent of DOP penetration before test was measured to be 0.002 and after the test the penetration was measured to be 0.28. You will note that a small hole appears to be melted in the media and separator close to the bottom edge of the unit where maximum heated air temperatures occurred. You will recall that our present proposal for percent of DOP penetration following this test is 3.0.

Slide No. 10 shows a unit subjected to the Low Temperature Test and slide No. 11 is a sample subjected to the Moist Air Test. In both cases, there was no change in percent of DOP penetration measurements made before and after the tests.

Slide No. 12 shows the upstream face of a unit subjected to the Spot Flame Test while slide Nos. 13 and 14 are close-ups of those areas which suffered the

greatest destruction during the test. It was noted that no burning, scorching, or discoloration of the downstream face of this filter unit occurred and reviewing the sample from this face, no evidence of the Spot Flame Test can be detected.

As I have mentioned, a draft of our latest proposal is now in the mail and we anticipate final action on the proposal in the very near future. As compared with our present requirements the proposal represents a reduction in the severity of our test program and we do not anticipate too many objections. As I have mentioned, and as can be seen from the slide illustrations, representative production samples of filter units now being supplied to your facilities appear to comply with our current proposal and if these requirements are adopted it is expected that high efficiency air filter units bearing our Label will be available in the near future.

LRL PANELIST: This work was an effort to carry on from that which Mr. Palmer did at Hanford several years ago. As nobody else took up the work, Mr. Gilbert asked me if I would perform tests on some filters that had been developed in the meantime. I was able to have some sheetmetal work done officially; we assembled equipment that we hoped would work for a test similar to Mr. Palmer's.

However, when we were ready to test the filters Mr. Gilbert felt that the testing at elevated temperatures was more important than testing for fire intrusion, so the tests were conducted with equipment that wasn't exactly designed for the kind of test that we tried to make. We are hoping that future tests after modifications to the equipment will be more precise and reproducible.

However, I do feel that we did gain some information from the series of tests last June, and are looking forward to continuing the tests at elevated temperatures in the future.

I have a few slides that show the equipment and the result on one kind of filter. The first is a diagram of the equipment. On the far left is the housing to hold the filters without a positive clamping device, but the filters are set in against a bearing surface for the gaskets. The five gas-fired burners can be adjusted for flow and temperature; any one or all can be used at a time.

We can use up to 24 thermocouples as required and record on a strip-chart recorder. The other parts are: viewing ports; Pitot Tube to indicate the flow rate; a slide valve to adjust the flow rate; and, a Lamb-type air mover to provide suction for the system. The eductor eliminated the problem of high-temperature gas going through a centrifugal blower.

The next slide is a close-up of the apparatus, showing the burners and their positioning. The left side of the system has been removed to show the filter in position. The track that holds the burner on the left side is missing, so the burner assembly must be held in place. The marks on the side wall are placed to give us a reproducible position of the burners from the face of the filter.

The next slide shows one of the filters tested. This was a special filter with the frame made of "Novaply," a chip-board material. Otherwise, it is pretty much of a standard filter, with a rubber cement, glass-fiber filter medium, and aluminum separators.

After the test was over and the DOP tests had been run on the filter we cut it up, and took a close-up photograph. This is on the upstream side,

showing some charring of the frame material at the upstream side, and some surface charring of the rubber adhesive, but the adhesive was still in pretty good shape. You could tear the filter media or aluminum separators before you could loosen them from the frame. This indicates that the seal was still essentially intact.

The next slide is the downstream face of the filter, 180° from the last slide, showing that the gasket material, the filter media, and the adhesive were all in good condition.

The next slide is a trace of the various temperature points. The one on the far left indicates the downstream total air temperature after dilution. The thermocouple was placed downstream almost to the air mover. The three on the far right were at the top of the filter, on the face towards the burner. The next two were on the face of the filter about half way down. And the one at the bottom of the filter was on the side toward the burners. The horizontal lines are increments of 30 seconds, indicating that the filter test was something in the order of five to six minutes. The lines are a little erratic and we do not feel that they indicate a good, reproducible type of test. We hope to improve these in the future and continue with the tests for evaluation of other filters.

Several other filters were tested in a similar manner. Most of them held up well, some perhaps a bit better; some a little worse.

An additional benefit from the test was for our Fire Department members of which observed and participated in some demonstrations. We first used an old CWS type filter, one with cellulose-asbestos filter medium and kraft-paper separators, and fired it. After taking the burners away, it continued to burn quite rapidly. The Fire Department had an opportunity to try to put it out. They used powder, CO_2, foam, and portable water type extinguishers. The only suitable way to extinguish the fire was to take the filter out of the system, tear it apart with an ax, and douse it with a bucket!

We then put in one of the newer style filters with a retardent-treated plywood frame, newer adhesives, asbestos separators, and glass-asbestos filter medium. It was fired in the same manner. After removing the flame it was completely self extinguishing in a few moments. The Fire Department is happy that the filters installed at LRL are of the new type!

UKAEA PANELIST: Rather than commenting on the presentations, just now, I would prefer to say a few words about the UK approach to the problems of filter designs, specification, maintenance and so on. I think we have in some respects a rather different philosophy of approach to the use of filters and their design. We have been talking about the wooden-framed 1,000 cfm type filters. We would regard these in many cases as inserts for use in cases. The first thing I think I would like to say is that we don't use extensively, at all, any filters other than metal-framed filters. That is to say, the insert is contained in a metal, rectangular frame, and the whole of the construction, other than the paper and the cement, is of metal construction.

There are slight differences to the design in detail. One particular one, I think, that would be worth mentioning, if you can remember the last slide that the UL Panelist showed, was a detail of the folded material with the corrugated spaces between. In our designs we have a rounded, beaded edge along the front of the corrugated spacer where it is in contact with the paper. I suggest that this might be one of the locations where you get pinholes occurring.

In using what we call non-combustible metal filters there are differences in our installation philosophy. In some cases they are used in what I call wall frame construction, which is common over here, but in other cases we tend to use inserts put into canisters. There are two ways of doing this, or three ways altogether. One is to put the insert into a case and seal one of the leading edges into a frame inside the case. The cases are metal, and they have conical ends. This is to say that the residual sealing problem, when installed, is simply that of making a seal in a circular section duct. I know this construction is more expensive than the simple, rectangular frame. These units can be of a type in which the whole canister with the contained filter is thrown away after its period of use. In another form the insert is capable of being taken out of the canister and the canister is re-used, with a new insert. With the third method there are end pieces which reduce the square section to a circular section clamped to each side of the insert, upstream and downstream.

We have been evolving specifications to cover the supply, manufacture, design and testing of these units. In fact, at the moment, we have specifications to which manufacturers are expected to supply inserts both 1,000 cfm inserts and the 200 cfm size.

The specifications include: dimensional tolerances of the cases; the type of design for sealing either into the canister or into the framework; the penetration and the pressure drop; the maximum mechanical strength across the unit; temperature resistance; the amount of filtering area used; the type of spacers and their design; the general construction, material and rigidity of the frame, particularly the flange faces; and, the cement which is used. Separate specifications cover the paper and gasket to be used. By that I mean the gasket by which the insert is sealed into the frame or canister, not that between the paper and the frame. These specifications cover both the throw-away type of unit and the replaceable types of unit.

We have codes of practice which are, to some extent, still in the course of development; the codes include the canisters I referred to of both replacement and throw-away types and for the end pieces which are used in some cases. We are hoping in the not-too-distant future to develop and to apply a similar type of specification for pre-filters to be used in conjunction with the high-efficiency units.

I would like to add that we distinguish, in the testing of these units, between proof tests which are used to test each unit, and type tests which are not done on each unit, but are done in order to satisfy one's self about the particular supplier.

As far as maintenance is concerned we feel that one of the advantages that comes from the use of the canister-type filters, is that unlike the wall units it is possible in almost all cases to work with fairly simple methods for breaking flanges with a sound technique and a minimum amount of protection for the operator.

PANEL CHAIRMAN: Thank you; we appreciate the viewpoint of the U. K. Atomic Energy Authority on the problems which are rather common to us.

The work that Mr. Horn is doing at Underwriters' Laboratories is prerequisite to the establishment of a federal quality products listing, or QPL, for a military filter specification. When the QPL is established we will recommend the new specification for a filter which is MIL-F-51068. This is a specification drawn together out of the experience of the U. S. Navy, the U. S. Army, and ourselves, and some research work done by Arthur D. Little over the period of the last three years.

There are four basic differences between the one which you know as the AEC Health and Safety No. 120 specification and, briefly, these are they: Filter efficiency will be 99.97 instead of 99.95 percent. In other words, penetration will be reduced to 0.03 from 0.05 percent. The allowable resistance for a new filter will be 1.0" water gauge, compared to 0.9". We have been using 1.0" for the past two years because of the insistence on greater tensile strength in the media, and on water-proofing, which have their effect on resistance. Thirdly, the new specification will require that the filter be tested at 100% and 20% rated flow, and this is in deference to those installations using a 1,000 cfm filter at 300 cfm; the pinhole effect, large or small, will probably be significant. Finally, filters with a face size 24" x 24" will be equipped with a 1/4" mesh hardware cloth protective screen over both faces. This is a damage-prevention measure. In the tests run at Underwriters' Laboratories, it has proved also to have just a a little bit of effect as a fire-screen.

Here is a small demonstration unit made up to the specifications of MIL-F-51068, and you appreciate that it has the hardware cloth face on it. This would not be required on those units which are less than 24" because damage experience does not show the need on units which are 12" x 12" and such smaller sizes.

One item on results on the Quality Assurance Stations at Oak Ridge and Hanford. Table I shows percentage of total filter rejections which were given to you in 1961 at Brookhaven, for the first half of 1961, and those for the first half of 1963. It is rather obvious that the improvement has been great in the quality being delivered to the program, as reflected by Station tests.

TABLE I

AEC FILTER TEST FACILITY RESULTS

First Semi-Annual Period	Percentage of New Filters Rejected, By Cause: Manufacturing Deficiency	Excess Penetration	High Resistance	Carrier Damage	Total
1961	1.1	3.5	9.3	1.0	14.8
1963	0.1	0.5	0.5	0.2	1.3

I find that the volume of high-efficiency filters coming into the program has increased about 60%, that will be the increase for this year. I do not find that the requirement of reactor confinement or containment accounts significantly for the increase. It seems that more filters are going to the same purchasers, which raises a point that has been brought out in the last three meetings. "Have you examined your airstream to be sure that you are required to use a high-efficiency filter?" This should be done. These are expensive filters, and where they are not needed, certainly we would hope they would not be used. Sometimes filters of less efficiency might be put in place.

QUESTION: I would like to address a question to the Chairman relative to the military specification. One pertains to the gasket thickness. I note UL mentions the 3/16" gasket. We prefer a thicker gasket because of the variations in the surface, in a practical installation, to get a seal. As an example, we found that it took a 700-pound load on a 24" filter to effect a seal. To overcome this, at least in part, our specifications call for a 3/8" thick gasket, unless otherwise excepted by the individual specification.

I am curious to know if the specification for integrity of the frame will be included in this Military Specification or anything regarding the tightness or integrity of the frame construction itself?

PANEL CHAIRMAN: Well, the recommended specification on which you purchase would be the military specification. The UL Standard would, of course, conform.

The gasket in UL says "not less than 3/16 in." I was looking for the provision in the Military Specification. I don't find it just now but my impression is it is a $\frac{1}{4}$" gasket.

The third amendment to the Military Specification is now under way, which has to do with the item you mentioned about frame integrity, particularly at the joints. We found that the specification was deficient in that it required no adhesive completely across the inner face of the frame, as well as the joints; but insofar as the frame joint test, there is no provision for it.

QUESTION: I would like to ask the UL Panelist two questions. When will the qualification program be in effect for UL? Second, since most of the tests so far have been made on plywood frames, are these tests equally applicable to the metal frames?

UL PANELIST: I would like to be able to say that we can get labels by perhaps January 1, 1964. On the other hand, I have been at this five years and I have learned to be very cautious about statements. But we do think we are much closer than we ever have been to completing a program and getting labels, and my guess is if we are lucky it may be only a matter of two or three months.

As far as the plywood and the metal frame tests are concerned, we have experimentally tested one unit in a metal frame, but it is the only experience we have had with a metal frame. Does that answer your second question?

COMMENT: Primarily it does, but we do have the problem that a good many of our frames will be metal, and we just wanted to know if the tests will be equally applicable at the time the qualification program goes into effect.

UL PANELIST: Yes. As far as we know there is nothing in our Standard that says you could not use metal, and I believe it will pass as well as any of the other frames.

QUESTION: I would like to direct this question to the Panel Chairman. With respect to this military standard you quoted a specification value for rated flow of a maximum penetration of 0.03 percent. Has a value been set up for penetration at the lesser rate of flow?

PANEL CHAIRMAN: I think that it is in the specifications here. The specifications were handled by the Specification Group at Edgewood, and were of primary concern to the Engineering Group. I would have to check the item in the document, the specification, before giving you an answer.

QUESTION: I noticed, in examining the color slides by both the LRL and UL Panelists, that there was a very perceptible difference in the appearance of the filter, top to bottom, on the High-Temperature tests, and I noticed, in the LRL curves of temperature, that there was approximately a 500° differential between top and bottom. First of all, for temperatures specified at some particular value, is it the intent that the highest temperature recorded on the filter will be the one that meets this test? Should an average temperature be considered; how will this be arranged?

If there is a single filter in a duct, then obviously the temperature differential which observed is perhaps a fair test, but would it be a fair test if there were a bank of filters, perhaps eight or ten high? In that case, the very top filter would be subjected to the highest temperature over its entire face. What effect would this have on acceptance tests?

LRL PANELIST: As far as our tests were concerned the equipment was designed for fire intrusion. We tried to put together a system for checking at elevated temperatures. The equipment, I hope, will be modified so that we can eventually get fairly uniform temperatures from top to bottom.

As you observed, in a bank one filter at the top will probably be fairly heavily exposed, and the one at the bottom will not be. This is pretty much the same as one filter in a test being hot at the top and cold at the bottom. It makes little difference in that respect except that, in the bank of filters, the top filter will be exposed over its complete periphery instead of just a portion of it.

These tests were not designed, and never will be designed, for acceptance tests; they are only for experimental filters to see how different equipment or supplies will work under some given set of conditions.

UL PANELIST: I would like to say that I agree with the LRL Panelist. At least the UL approach is to test a filter as a unit and obviously we cannot be concerned too much with how these things operate in a bank. In the test apparatus that we have designed, we are attempting to operate at the maximum temperature; not the average temperature, but the maximum temperature, at 700°F., and that is why you noticed the filter samples had less of a heated area at the bottom of the filter. It is a very tough job in a reasonable filter test duct to get uniform spread of heat, and we have been fighting this thing for a long time; the pictures you saw are about the best we have been able to do so far.

PANEL CHAIRMAN: I would like to augment that. From the tests at temperatures higher than the 700°F. where we had penetration through the seal, you might say it appeared that the thin veneer layer of plywood was involved in the combustion at the juncture of the pack. This was one of the reasons that prototypes were tested at Livermore. One of these prototypes was made with "Novaply," which is a trade name, or chip-board; but I guess the generic definition is particle board. It appeared, this is not conclusive, we might get more fire resistance in the frame, in the wood frame, from fire-resistant treated particle board than from the treated AA exterior grade plywood. This still is under examination. The higher temperature at any one point is the one, I believe, that Underwriters' Laboratories has always construed to be the upper limit of the test.

QUESTION: I would like to direct this question to the UL Panelist. I believe you mentioned setting up some sort of a 3.0 percent penetration limit on flame intrusion test. What is the basis of this value?

UL PANELIST: I was afraid someone was going to ask me that. I think I will have to ask the Panel Chairman for his explanation of the basis. As far as Underwriters is concerned, we have leaned very heavily upon the field experience that you have had in this area, and in trying to evaluate a spot flame test it was very evident the filter was going to fail. The temperatures are high enough to melt a hole in the filter by this method. If this is the Heated Air Test that you were talking about, then it is the 700°F. test and it was recommended by the AEC that perhaps an efficiency of 97.0 percent would be satisfactory.

PANEL CHAIRMAN: The tests that have been run at 700°F. show no penetration thus far greater than 1.0 percent. I think our philosophy is to live with a fire-resistive filter; not fire-proof or non-combustible, because of cost. We can do this, but we want the maximum barrier, so we have an arbitrary figure of 3.0 percent penetration through such a filter after a Heated Air Test for five minutes at 700°F.

COMMENT: The experience at the Hanford Filter Testing Station on filters that I have seen going through, and others could elaborate on this even more, we are getting an increasing number of metal-frame filters. I would guess the figure is now running somewhere between 25 and 30% of the filters coming through. Many of them seem to exhibit what appears to be a weakness in the frame joint, which we certainly have undergone on the plywood frames in the past, but the metal ones certainly seem to lack rigidity. These do, however, pass the standard DOP and resistance tests in many cases. We have also had some notable failures.

With this, there has been quite a trend among these metal frames for them to be stainless steel, which cost-wise, is a tremendous increase. I would like to hear some of our contractors answer why they are buying metal frames and why all the stainless steel?

BNL COMMENT: The main reason we are buying stainless steel frames is the problem of waste disposal. We want something we can use again. We take the filter media out, clean up the frames, and send them back to the manufacturer where they are rebuilt. We can then package the filter media in a small space for waste disposal.

COMMENT: This is an interesting point; I have never heard this reason given. The latest comparison I have seen shows a cost of $43 for a 1000 cfm plywood frame unit with the stainless steel listing for $525. I don't quite see the economics.

BNL COMMENT: I guess you can buy them at that price. We have not paid any such price for ours; the ones we are buying are about $120. We are returning the frames, and then our new replacement filters actually cost less than if they were plywood. There is an initial investment, but when you consider that you have to pack these used filters and haul them anywhere from 100 to 400 miles to get rid of them it is a real saving if you can bale them and decrease the space; that is why we think we are making money on this.

PANEL CHAIRMAN: I think that the activity level also might be a factor in refilling a frame.

UKAEA COMMENT: Well, as far as metal-framed filters are concerned, I would make it clear that the metal we use is mild steel, but they are, of course, more expensive than the wooden ones. We recognize this, but we are willing to accept this extra cost for the advantage we think it gives.

As far as the sealing problem is concerned, I would agree that there is a sealing problem which can, we feel, be overcome. There are two methods known for doing this, and we have in fact tested both, which we regard as satisfactory from the point of view of the seal of the filter material to the case; we have, in fact, tested such filters which have been in use for some period of time.

But, I would nevertheless agree that a lot more work could be done to evaluate the satisfactoriness of the seal after use.

PANEL CHAIRMAN: I have an answer for a question posed earlier concerning the percent penetration at 20% of rated flow. The percentage limit is also 0.03, the same as the rated flow.

COMMENT: At Hanford, we have had a little experience over a period of time with respect to the penetration of the over-all filter, including gasket leakage and frame leakage, in addition to the filter penetration of the core itself. I think I am obligated at this point to indicate that we had a shipment come in just before I came on this trip and it exemplifies the point I am trying to make. These filters were good filters based on visual inspection; they were well made and it was obvious there was care taken in their fabrication all the way through. But the design was such that it made us question whether or not the overall penetration might be as good as it was supposed to be. We ran the regular performance test and found that they were well within the 0.05 percent maximum penetration limitation. We have a modification on our testing facility which can demonstrate the overall penetration of the filter, including the frame-joint leakage. As we suspected, we could put them on the floor and lean on them, exert a little pressure, and they would sway back and forth out of the 90° angle; this was true even with those of exemplary construction.

With the half-dozen or so filters that we tested, the highest one had overall penetration about 3 to 5 times the penetration of the core. This is an indication that leakage does go high. I think this is something that warrants further consideration in the program, where the overall penetration is of consequence and can be serious in application.

COMMENT: Another comment to add to the metal-frame problem relates to my former employment at General Atomics. We had satisfactory experience with wooden frames through several filter fire experiences, and yet, due to conservatism, operations personnel insisted on ordering metal-frame filters.

I am also interested in a case of fires of filter loading, whether aluminum separators will stand as much as the asbestos ones. My feeling would favor, perhaps, the asbestos; I do not know what the test would be.

COMMENT: If you can promise you will have a low-temperature fire, the aluminum separators would be all right. In fact they would be better, because they tend to distribute the heat through the filter evenly; we found this in the test at Underwriters in February. But, if you have a high-temperature fire, and this is the usual case - a fire in the range of 1250°F to 1500°F, the filter with aluminum separators will carry away almost explosively because they melt so fast. There the asbestos separator certainly is an improvement. I will admit I belong to the asbestos separator school, rather than the aluminum.

PANEL CHAIRMAN: Asbestos mineral needs a binder of some type to hold together in the form of paper. We have run an analysis of asbestos paper that has shown as much as 29% organic binder. You might keep in mind that fact when considering which separator to choose.

I would like to digress just one moment before we close. We have been considering the combustible make-up of filters for quite a few years, stemming primarily from the early desire to incinerate the cellulose-asbestos types, and recover the then scarce uranium metal. There was a research report at one time which had a page and a half to the effect that if the filters can be incinerated, they can also burn in place. This was patently ignored for quite a few years although we had glass paper available for filters. Mr. Palmer

should get a lot of credit for bringing the attention of this problem of filter combustibility to the atomic energy program; he worked under very difficult circumstances to make a small voice at Hanford known to a fairly large program. He did the early fire tests under quite difficult conditions, as Mr. Murrow indicated, sometimes unofficially. Frankly, the atomic energy program is indebted to him for his earlier efforts, and I rather suspect that the Western World is too.

May I take this opportunity to thank all of the panel members for their excellent presentations. We appreciate their efforts and thank all of you for your questions and comments.

PANEL C - ROUND TABLE SESSION
Thursday Afternoon, 24 October 1963

OTHER KINDS OF FILTERS, EVALUATION METHODS AND PROBLEMS

a. Super-efficiency
b. Roughing or Prefilters
c. High Temperatures
d. NaK, Na, Special Problems
e. Other Test Methods

PANEL CHAIRMAN: We have a rather interesting panel session in Panel C, Other Kinds of Filters, Evaluation Methods and Problems. I would like to comment that we regret one of our panelists, Mr. D. E. Fain, ORNL, for reasons beyond his control could not be present this morning. Therefore, the final composition of the panel is:

L. Silverman, HACL, Chairman
W. E. Browning, Jr., ORNL
R. G. Dorman, UKCDEE
W. J. Megaw, UKAEA
A. H. Peters, SRP

The purpose of this panel is to discuss problems of the future as well as those of the present. We will follow the procedure of previous panels and allow the panelists to make brief statements and follow their presentations with questions and answers.

ORNL PANELIST: We have made some theoretical calculations in which we have tried to form an idea of what will happen to materials after they have been released in a reactor accident. We have tried to take especial advantage of certain physical facts; namely, that they do occupy space volumes and there is only a certain amount of meterial depending upon the volume capacity of the particle.

The other physical property is the tendency of particles to agglomerate as a result of their colliding with each other. We tried to set some theoretical limit, if we could, as to the amount of radioactive material which could be contained in gas suspension as the function of time after release.

Mario Fontana, who is the co-author of the paper ORNL-NSIC 1 applied the expressions in Slide 1 to the case in which a reactor is contained in two concentric containments, the outer one of which is ventilated.

If you are talking to members of the Advisory Committee on Reactor Safeguards, who are very skeptical about the reliability of your systems and you have been testing your filters with DOP aerosols, you might find it difficult to convince them that youhave a filter that is good for anything other than the aerosol size for which you have been testing. It turns out that you can forget about particles of any different size than 0.3 micron as long as you catch them when they get to be 0.3 micron. The alternate is the number of curies released. I should mention that we are assuming here that the particles are loaded to their full volume capacity with iodine atoms.

If you are convincing people that you can remove 0.3 micron particles, then the critical time is around 20 seconds; this has a value of approximately 30 seconds. That means you have 30 seconds worth of leakage which will be released during all the time after the reactor accident; if that is satisfactory you do not need to make any more careful analysis. These highly pessimistic assumptions are sufficient.

Applied to the N. S. SAVANNAH case, this means 10^{-5} of the total iodine would escape. While this is just a theoretical treatment, if you want to be really positive, there ought to be some kind of experimental work done. But, it does look to me that the limited volume capacity of particles and the limited concentration that is stable against agglomeration can be used to set a maximum limit as to the amount of material that can be contained in small particles in a gas beyond a certain time.

PANEL CHAIRMAN: There is a subtle implication here as to whether this is a super-efficiency filter, the point being that if you are depending on agglomeration you can, in fact, get higher efficiency from an installed filter. I will have a little more to say after the other panel members have completed their discussion about higher-efficiency filters than what we are using.

The SRP Panelist comes next on the basis of some discussion of roughing filters, demisters, and problems associated with them.

SRP PANELIST: Yesterday, Mr. List described the containment or confinement program at the Savannah River Plant for our production reactors, and he briefly mentioned that we use demisters or moisture entrainment separator systems. I am afraid that I cannot tell you today whether it is economical to use roughing filters or prefilters in any given containment system. In our own case the benefits we get from the demister as a prefilter is only a bonus feature. The demisters are absolutely essential in our own case to protect the particulate filters in the event that we release large quantities of steam in a nuclear action. We have found that dusting filters fail seriously when exposed to unprefiltered mixtures of steam and air containing entrainment, and therefore we have installed or are currently installing moisture entrainment separators. We have found that these units also filter out a certain percentage of atmospheric dust. We have measured in our laboratory program removal of about 30 to 35% of the atmospheric dust by the demisters.

The moisture entrainment separators consist of the conventional stainless steel wire pads, but they differ in that each wire is wrapped with a teflon yarn containing 60 individual filaments of teflon. The individual filaments are 0.8 mil in diameter. Each demister is 2' x 2' x 2" thick. We use 5 demisters upstream of 8 particulate filters. This gives us the required superficial velocity to remove the particles of entrained water in the steam-air mixtures. About three pounds of teflon material are contained in the demister. The density is about 30 pounds per cu. ft.; each demister is rated at 1,600 cfm at a pressure drop of 9.5" of water. I might add that we find the demisters remove approximately 15% of 0.3 micron DOP particles in a standard test.

The units in our own application are expensive in that they are contained in stainless steel cases, and this about doubles the cost. The price is $200 per unit in quantities of 200 or more. The demisters, without the stainless steel case, in such quantities cost about $80 per unit. This is a high initial investment. We use the stainless steel case because we have found that we can clean the demisters simply by vacuuming off the upstream face which collects the cake. There is not an appreciable penetration of the atmospheric dust into the bulk of the fibers, but with time, there will be

penetration. We feel that this dust can be removed by back-flushing the demisters with water.

At the present time, we have an experience of about 13 months in our laboratory facilities. The pressure drop during this period of time has increased from the initial value of about an inch of water to four inches of water. The vacuum cleaning technique produced a pressure drop to within 25% of the initial value.

As far as the production facilities are concerned at the present time, we only have about five months experience, and it is too early to tell whether the demisters have made any beneficial effect in reducing the pressure drop on the downstream particulate filters.

As was mentioned earlier today, we are finding very high pressure drops on the particulate filters after about a year's exposure, approximately four inches of water. This is due to an unusual condition at Savannah River and we attribute it to fly ash from nearby boiler stacks.

A very rough prefilter is in the supply air system. We have a study underway at the present time to determine just what is wrong with these roughing filters, as far as removal of the fly ash, which we have positively identified. We suspect that the filters have not been installed properly; they are of the paper type that you just fold over and insert into a frame.

The work on the demisters is reported in detail in DP-812; the September 1963 issue of Mechanical Engineering covers the work in summary form on the demisters, particulate filters and culminate filters.

UKAEA PANELIST: I was very interested in the ORNL presentation a few minutes ago, because in our reactor containment tests we found out that some iodine does go onto particles; and, as far as we can make out, they all seem to be particles of about 0.5 microns in diameter. However, the iodine is also irrepressibly absorbed onto these particles. You cannot get it off by increasing the pressure or by drawing clean iodine through a Millipore filter containing the particles. We think we are on the scent of something at this stage, and we tried to make iodine attach itself to artificial aerosols. In metallic aerosols the only success we had was with silver. We got some reversible absorption, but not very much irreversible absorption. So it appears that there were some particles in the atmosphere to which iodine absorbs, and the problem is to find out what they are.

The first point I should make is that if this really does occur at 0.5 micron particles, then, if we are going to release iodine from a reactor, we had better make sure that it is attached to the small particles so they don't fall out under gravity at all to any extent. The sedimentation velocity is very slow. They are so big, molecularly speaking, that they don't diffuse out at all rapidly. I think that iodine in this form is in the best form to release into the atmosphere. The deposition risk will certainly be greatly reduced, and the inhalation risk will be reduced somewhat. Chamberlain some seven or eight years ago did some work, which showed that the deposition of thorium-B on the tracheae and bronchi was reduced by a factor of several hundred when the thorium was attached to these condensation nuclei. The sort of particles that are in the atmosphere in the sub-micron range, we have looked at from time to time.

I think particles similar to those shown in three slides may be of some use in filter testing. I know that the DOP test was chosen because of the size 0.3 microns, with expected maximum penetration; it is also a reproducible aerosol. One criticism of this method might be that we do not have a great deal of notion

about the size of the particles, unless we go to the trouble and expense of taking samples for electron microscopy. On the other hand, we have shown that we can get a fair idea of the size by using diffusion techniques. An even simpler way is to measure the fraction of the particles which is not charged. The theory of it is a bit dicey. Somebody asked Professor Shredding a few years back before he died if there was any basic theory in it and he answered there was no earthly reason why the theory should be true, but if it seemed to work, there was no harm in using it.

This method has been pioneered by a number of people in Ireland and in America. It works on the application of Boltzmann's distribution law. It can be written as an expression for the number of particles carrying two electronic charges. This agrees very well in our experience with the size that you get out of a diffusion battery measurement. We have checked this to a size of about 0.5 microns, and there we had some difficulty, but we think it was due to our difficulty in getting a good aerosol. We took it up again at 0.5 microns and worked it up to just above 1 micron. There, the series changes so slowly that the measurements don't really mean very much. But for measurements, say between 0.01 and 1 micron, this measurement of the fraction of an aerosol charged gives a very good idea of the size of the particles, an average size. It must be brought to charge equilibrium, because if you generate these aerosols by spraying or by heating, you may have some particles with very high charges. But if you store the aerosol, natural radioactivity, cosmic ray and ionization soon brings the aerosol to a state in which there is no subsequent change of charge pattern with time. During a study of dynamic charge equilibrium, this process can be accelerated by passing the aerosol past a radioactive source.

I would now like to talk a bit about polystyrene spheres. We get these gratis from the Dow Chemical Company at Midland, Michigan which is an advantage, although the requests have been so great that there has been some delay in sending them out. But they are beautiful, uniform spheres. They are uniformly within very near limits, indeed. We would like to dilute these so that in theory one droplet can only contain one polystyrene sphere. But in fact, we do get clumps of spheres. After we filtered it through an atomizer we picked out the big clumps in a cascade impactor, then dilute the stream quickly so we don't get much coagulation, pass it into a 100 liter vessel to allow some drying. One drawback is that no matter how pure the dilution water is there are a tremendous number of very, very small particles, less than 0.01 micron, sprayed up from the solution. We take these out by passing them through a very coarse filter packed with copper knit mesh. This takes out all the small particles, but it also takes out about 50% of the polystyrene spheres; but the aerosol obtained at the end contains less 1% by number of the very small particles.

These things, I think, will be quite useful in filter testing. You can get them in sizes up to a micron, or thereabouts. Above 0.3 micron it is rather difficult to get an aerosol that is sufficiently concentrated to be useful; but, in this range, certainly 0.388 is a very amenable size to use. You know the size of particles that can be tested and the concentrations can be measured quite easily with nuclei counters, which were described this morning. I would like to suggest this to the meeting as a useful means of testing filters.

These must be brought to charge equilibrium because, as you spray them up, you get particles with very, very high charges. For instance, if you measure the size in a diffusion battery before you reduce them to charge equilibrium, you get answers which are a factor of 3 off, or so.

PANEL CHAIRMAN: I just want to warn you that almost ten years ago they were using polystyrene latex spheres here for test suspension with a P-32 source to discharge it. But it is good technique and a lot of other people in this

country have been using this aerosol.

Our final panelist is from probably the heart of the early British aerosol work, the U. K. Chemical Defense EE Center at Portron. Many of you are familiar with Portron; I cannot make a comparison with Edgewood Arsenal, but it is the same kind of facility basically. He has some interesting information, and what he reports on will be the next standard test method in England.

UKCDEE PANELIST: In the time available I can only give you a short outline of the sodium flame particulate test. Slide 1 is a schematic diagram. We generate an aerosol of carbon salt in spray bottles. There are four bottles per 1000 cfm; the salt is 2% solution. It passes down the trunking, which is about a foot in diameter and some 30 to 40 feet long by a baffle, so that it gets adequate mixing before the aerosol reaches the filter, and the length of this trunking is sufficient to cause all the water to evaporate from the particles. We hope we end up at the filter case here with dry particles of carbon salt. This does only happen if the humidity in the trunking is below 60%. If it is above that drying is improper and we end up with a rather spongy aerosol. Some correction factors can be made if the humidity is known and we know the penetration has got to be reduced by a certain amount.

Now having passed through the filter, the aerosol goes out into a room. But before we sample the aerosol, we mix it by means of an orifice plate and a baffle. This means that it takes care of any pinhole leaks or any gasket troubles. We can sample anywhere in the duct and get the same takeup of salt.

The particles are pretty well all micron in size. They range from about 0.01, which is the limit of our electron microscope up to just a little over a micron. We get an odd one; one in every million or so goes up to 2 microns, or something of that order, but these are insignificant in mass and number.

We may sample anywhere across the ducting. We sample roughly isometrically, anything between a quarter and four times is good enough for tiny particles. We lead the aerosol through to a part of the circuit which is greatly enlarged. There is about a cubic foot per minute passing; anything between ten liters and 100 liters is quite satisfactory.

In a vertical tube is a small ceramic burner, burning hydrogen, and this is only looked at so that we see the 5890 sodium line. It is a free-burning flame, and a design of this size is sufficient. The flame doesn't flare at all. It burns just enough air to keep it burning freely. That means it sucks the salt laden air up, and the flame is colored yellow.

The multiplier is connected to a sensitive galvanometer and the increased deflection, because there is a standing deflection due to the clean flame, the increased deflection is directly proportional to the mass of salt which is burned in the flame. This is independent of particle size. It does not matter what the sizes are within our test limits. I wouldn't say that if we burned a 100 micron one, you would get the same thing as with lots of little ones; but, when they are all a micron and below, the deflection is proportionate to mass, depending upon particle size.

The calibration of this setup is by taking known volumes of the unfiltered cloud and diluting with clean air, and mixing, and then passing through flame. One gets a straight line of the concentration against deflection.

Slide 2 is an enlarged diagram of the photo-multiplier housing and flame tube with a tube leading the hydrogen up. The glass chimney has a silvering on the inside. This silvering is cut away, so that the multiplier sees the flame.

The tip of that burner is about a millimeter below the hole so that the burner itself is not seen, only the flame, a bats-wing flame.

If the humidity in the ducting is not low enough, the salt particles will not dry and we end up with a larger aerosol than anticipated. If the particles are too large because of this increased humidity, they do not penetrate so easily. We have bought a drying apparatus because we are performing these tests at regular intervals, and we wait until the air is dry enough. A drying apparatus is not too expensive and it adds something to the cost; we can test whatever the weather is. I suppose this is perhaps more important in England than it is in the States.

Perhaps the thing that is of more interest to you is the comparison of results between the sodium chloride tests and the DOP tests. We are building a DOP rig. We had one some 20 years ago and dismantled it. However, we now think perhaps we ought to do some correlation.

For the purpose of this comparison, I have taken some results on small sheets of paper from our Canadian and United States friends. I cannot vouch for them. I can't blame them if they haven't given us the right answer, but we fully expect they have given us the right answer. I think we are getting the sort of comparison we should expect.

We found in one series of tests on 4 square centimeters of glass fiber paper at 13 centimeters per second that something on the order of 2.05% penetration, but the sodium chloride cloud was more penetrating by a factor just over 2. This is at a distance of 13 centimeters. But when we go down with similar sorts of paper about 2.5 centimeters per second, which is the velocity per 1,000 cfm filter, I think, the DOP penetration is about two to three times bigger than the sodium chloride. This is most unfortunate, but we can still measure the penetration by salt down to .201 at 1,000 cfm, with something like $\pm$ .302 or .303 possible error. But it is a less penetrating cloud velocity, and I think this is what one would expect. The lower the velocity, the more highly penetrating would be the DOP compared with the sodium chloride. These, of course, were on small sheets of paper; they were not on large filters.

Slide 3 is a photographs of our rig. Here is the spray box. We blow air down here, right down the trunking, with a bend in it at its base. Here is a filter under test showing the monitors. We measure flow and various things by putting these across an orifice plate. This is a flame tube. There is the end window multiplier. This inclined gauge measures the flow going along this tube. It is a rather crude method, but we save money whenever we can. The power unit is a discharge stabilizer with fixed voltage.

Slide 4 is a close-up view of the spray box end. These are gauges for the spray. I don't think I mentioned it, but we spray at 100 pounds per square inch; and, with four atomizers which you need for testing at 1,000 cfm, this involves using 12 cu. ft. per minute of free air.

Slide 5 shows a blower and filter here. We keep these going all the time, so that when we switch off the air from down this trunking, our little flame in the decanter over here is burning clean air, so it doesn't get contamination on the burner.

We can measure penetrations at 1,000 cfm of about .001. This is perhaps pushing it a little, but we would claim this if we were forced into that position.

Now finally, as with the DOP apparatus, if we can't find the signs of any leaks, we disconnect the trunking behind the filter and we put a searching

tube across the face of the filter. We can find exactly where the leak is in the filter.

I might say we've found quite a lot of leaks in the commercial filters we have tested.

PANEL CHAIRMAN: I think the ORNL Panelist had a little concern about super-efficiency; I consider him as one of the advocates of the need for super-efficiency filters. If you can get away with the containment vessel as a replacement, I am all for it. I would like to say though that we do have some situations where filters of an efficiency more than .02 or .01% penetration on small particles may be needed, and I would rather let these instances come up with the hope that we have a solution for them. In fact, we have been asked for a solution on previous occasions. We are using bag filters in which an asbestos float is atomized or aerosolized, if you will, into the air stream. Deposits on the surface of the filter produces an additional filter layer, or a "schmutzdecke," as the Germans would call it. This will then give you a very high efficiency filter.

In the work we did at the Harvard Air Cleaning Laboratory several years ago we found that we could correlate the thickness of the deposit with the improvement in efficiency. So those of you who feel that the absolute filter isn't absolute enough, that would like to get higher efficiency, if you will aerosolize asbestos floats into your duct system, you will get an improvement in performance, and you can show this with DOP or salt, or oil smoke, as we did. I think that in the operations at the beryllium plants, this has made the difference between a very expensive filter installation, where the filters would have to be thrown away, and one where you can use a bag filter and replace the surface after you shake it down into the bin. The so-called ultra air cleaner, which is sometimes used for pre-cleaning atmospheric air as well as the operations of an after-filter after all other filters, using asbestos floats is one technique.

The ORNL Panelist has suggested another technique; that is the setup of super-fine aerosol which will also build up a cake that will be highly efficient. I would like to know though, in terms of super-efficiency problems from our audience, if there are specific applications or questions in this area.

The next item is the question of high temperature. I think you all know the problems of the fire in an absolute filter system. The Dow people had one that was of significant magnitude, and the question of whether or not we need filters to operate continuously at high temperature, not just fire-resistant, has been a major problem.

I think I indicated yesterday with regard to the diffusion board, that it is in fact a high temperature ceramic type filter. There are others that have been demonstrated and used at the ANP project and other places, and if there is still some interest in this area, we would like to hear from the audience.

Another point is on the matter of roughing filters. The SRP Panelist has indicated the Savannah River experience, and I asked him if he knew anything about the comparison of life on absolute filters with and without prefilters, and Savannah River seems to be at some odds in this area, as compared to Hanford. Hanford didn't put roughing filters on their fire confinement system. I am not certain that the Savannah River had ever planned this, but in their metallurgical operations they always precede absolute filters with a dust stop or a similar filter. Most of the statistics we could gather in our survey indicated that this probably doubled the life of the filters. We had other

applications where this was apparent. So one has to get down to the question of absolute economics, which was mentioned yesterday, in that the prefilters might cost as much as the absolute filters; and the question arises, "Well, what is the point in doing it?" Or if one looks at a dust stop, which is a 50-cent or dollar filter, and get twice as much life out of the absolute, I think the economics are clearly apparent.

The other deals with NaK and Na, and special problems associated with them. I think this is one of our future concerns. We are concerned with the fast reactors; we are concerned with the SRE type, the sodium graphite reactor. We have had NaK fires in laboratories, and we have tried various solutions for this.

In our case, when we heard about this, we tried to develop a vinyl filter which could be washed, but this was not intended to be an absolute type filter. We have since seen that we could get paper made from Dynel fibers, or thermoplastic fibers, like polystyrene; there is not enough demand for us to have any filter manufacturer make it in absolute size or in quantity.

With the coming need for these type filters, especially if we go to fast reactors and must use sodium as a coolant, we will have problems where a filter must use sodium oxide or sodium potassium oxide, and we may need a much better filtration system than we have at the present time.

COMMENT: My comment isn't exactly relevant to your discussion, but I want to get it on the program somehow, and I think this is a propitious time.

Mr. Collins previously indicated that some significant portion of the iodine that is released will go into methyl iodide, which goes right back through the filter and is released; and, he indicated that the amount of methyl iodide that is formed seems to be related to the amount of organic material that may be in the atmosphere. So here is some kind of a correlation between a serious penetration problem, both with filters and with carbon beds, and an organic constituent in the atmosphere.

Mr. Brion indicated yesterday that some aerosols will decrease the filter efficiency; at least, in paper and cellulose filters. In our private discussion afterwards, we were speculating that because of the surface tension effect on the fibers, it might also depreciate the performance of other types of filters with other types of fibers. He indicated also in his private discussion that burdening a carbon bed with organic vapors, and he mentioned specifically paint solvents, would really interfere with the performance of a carbon bed filter.

Now I pose two situations where a burden of organic constituents could interfere with either a particular type filter or a carbon bed type filter. I am concerned whether anyone has looked at this problem to see what order of organic contaminants in this type of atmosphere would have what types of effects on the penetration of these types of filters.

PANEL CHAIRMAN: There is a difference of opinion as to the effect of tars on filters. Some of the work we did with incinerator effluents with tarring aerosols has indicated a serious plugging problem to the point where flow was practically decreased to zero, rather than a penetration problem. So I think you can get more than one effect.

UKAEA COMMENT: I think that having regard to the observations made by ourselves and at Harwell on the formation of methyl iodides where iodine is released into the atmosphere in various conditions, we ourselves have no doubt

that it can under certain circumstances represent a real problem, with which carbon filters are unable to deal. Undoubtedly, the ability of carbon filters to deal with methyl iodide, even up to the loadings which we find that they can effectively retain, may possibly be decreased by loading the charcoal bed with organic contaminants, which is obviously a problem perhaps of more concern to chemical processing operations than to reactors. But here we must not forget the work that is being done on organic reactors, where this may well be a quite severe problem.

COMMENT: At Hanford, I have looked at the effects of vapors on charcoal filters, and I find that the hexaclone does not interfere with the molecular iodine. I have also looked at the effects of oil droplets from aerosols, and there I find that this leads to increased penetration of the bed. I do not know whether it is because of the iodine going onto the droplets, or formation of iodine compounds.

AEC COMMENT: I have data to offer Mr. Dorman. On November 15, 1961, we checked a 1,000 cfm panel from Great Britain, which had been exposed to methylene blue with a cutoff rating of 0.01, sodium chloride 0.0035 penetration, and the DOP at Edgewood indicated 0.008, which was roughly twice the sodium chloride consistent with the particle size. I won't ask him to vouch for the makeup of the sodium chloride machine on which it was checked.

Dr. First, I think, had a question over here.

HACL COMMENT: I was going to accept your invitation to comment on fireproof filters by pointing out that the fire retardent resistance is largely a matter of construction, using fire retardent plywoods, rubber base cements, and so on. The upper limit is about 300°F, not that the filter will burn up at that temperature, but the organic materials begin to degrade.

On the other hand, using metal framed filters with glass fiber packing instead of cements and gas vapor made by asbestos or aluminum separators, the effective temperature limit is somewhere between 900° and 1,000°F., depending upon the resistance of the particular paper. All that will happen at high temperature is that the organic binders in the paper will burn off with a small amount of smoke, and the filter will degrade. For higher temperatures than this it is necessary to go to a ceramic material. It is possible to build a filter with ceramic fibers which will withstand temperatures to 2300°F. for an indefinite period. These were the filters that the Panel Chairman was referring to at the reactor test site, I believe. The primary objection to filters that withstand 2300°F. is their enormous cost, but there is a complete range of materials available where the need exists.

PANEL CHAIRMAN: Has there been any experience with mechanical failure of ceramic filters because they are brittle and cannot take much in the way of vibration or shock?

HACL COMMENT: Yes. I saw two installations at Merck Chemical Company about a year ago, in which ceramic filters were installed in a filter frame; I believe there were 16 of these units costing almost $1,000 apiece. No provision had been made for thermal expansion, and, when I saw them, each one of them was completely cracked and useless after about one day's operation.

COMMENT: What is the best means of testing carbon filters for iodine removal efficiency in-place? I don't know whether it is appropriate to this panel, but it doesn't seem to have come to an easy solution previously.

ORNL PANELIST: We have run a number of in-place tests for the SAVANNAH to

determine whether the charcoal absorbers are intact and functioning. In these tests mostly elemental iodine is injected into the ventillation stream about the place where the gas leaves the reactor compartment, and samples of this gas are taken before and after. The samples of gas are passed through charcoal absorbers and filters, and these collectors are radio-assayed to determine the efficiency. We have obtained consistently high efficiencies in these tests.

Sometimes it is inconvenient to perform radio-assay tests because of the possibility of contamination hazards. We have limited these tests to times when the ship is in a place where we can stand an accidental spill or at sea or berthed where there is enough confinement around the ship to prevent accidental exposure of the population.

For this reason, we have developed a method of testing which is similar to the radio-assay technique, except that we use larger amounts of normal iodine, without any radio-iodine, measuring by activation analysis. In the activation analysis we can detect a microgram of iodine readily, although background amounts of normal iodine limit our actual sensitivity to about 5 micrograms. This background iodine comes from the charcoal and possibly from the atmosphere.

COMMENT: I want to give a report from the stranded ship SAVANNAH. The methods just described for iodine checks work quite well when we are in port and we have every expectation of being out of port eventually. In the program, we have been working on instrumental methods of checking the filters, using stable iodine having cross-checked the instrument both with the Bureau of Standards and correlated it with the developments of some work that Mr. Adams has been doing at ORNL. It is not as sensitive a detector as is activation analysis. However, we can, on a go-or-no-go basis, determine the filter removal efficiency up to 99.8%. Since we are limited by hazardous evaluation to operate as long as the filters are above 99.0%, we feel it is good enough.

PANEL CHAIRMAN: Earlier this morning in the first panel, somebody had a question for this panel, and I haven't heard it repeated. I would like to have it now.

QUESTION: The question was with regard to testing in-place by means of stack monitoring devices. The sources suggested were possibly radon decay daughters and normal fallout that might be present, or possibly a known and controllable process stream being fed through the filters.

LRL COMMENT: The system using radon daughters and atmospheric dust as a test method to check filters was developed and has been used once since. There is no organized use of it in checking filters at this time.

QUESTION: The question I have relates to the use of pre-filters. Mr. Peters mentioned 15% DOP for the Demister. He also mentioned a 30% deficiency. He didn't mention what basis was used on that. If one were to specify a pre-filter to have a 50% DOP, is this a good way? Or should one use, say an NBS, either atmospheric or the dust? I am interested in the British use of pre-filters, what they have found in this regard as to recommended efficiencies and added life which you might expect toward the high efficiency filter.

UKCDEE COMMENT: I have some figures for pre-filters, which are glass fibers - rough glass fiber, 50% penetration initially to a standard dispersed cloud with a mass mean diameter of about a half micron. With this backed by what you might term an absolute filter against this particular cloud, the resistance area is about four or five inches, and the penetration of the filter had dropped 50% to 0.1%. Carrying out these experiments, we can use such a pre-filter. We can use about eight different pre-filters before the backing filter has risen

appreciably in resistance.

The use of pre-filters obviously must depend to a large extent on what the particle size is. Hard and fast rules cannot be made. We must know what particles are in the air before we can decide whether it is economically worth while to use a pre-filter. But it is surprising how a very, very poor pre-filter soon clogs up and becomes a good filter because of the deposition of particles on it. 50% from 1% is quite a large range in increased efficiency.

SRP PANELIST: We generally agree with Mr. Dorman. The 30 to 35 per cent I quoted on the Demister was for rural atmospheric dust at the works in the pilot plant area, with a particle distribution of about 1 to 40 microns, and a mass mean diameter of 3 to 4 microns.

I think the application of pre-filters has to be considered for each application. In our own case, I mention the fly ash problem, the mass mean diameter of the particles getting out of the stack and into the filter was about 3 to 4 microns. We feel there, that the Demisters are going to do a very good job as a dust cake is built up on the upstream face, and eventually extend the life of our absolute filters, which at the present time is very low - one year where we were hoping for three years. The Demisters we have a stainless steel case. We hope these have a life in excess of five years, since we have demonstrated they can be cleaned.

PANEL CHAIRMAN: Mr. Cheever's question as to whether or not DOP is a good test method for the other types of filters makes a good comparison if such equipment is available. But as to the correlation with NBS and any other test codes, there could be some doubt because of the size of those aerosols. They have a fairly wide size distribution, whereas DOP has a narrow spectrum, and complete correlation may not be obtained.

In screening materials a test like DOP has an advantage, in that some comparison is possible, but there is quite a shock when looking at something like the pre-filters of HB-2, and so on. When you run a DOP test and get zero for efficiency, these are oil coated filters and they don't respond at all to this particle size.

UKAEA COMMENT: To add to that comment on pre-filters, when used in buildings where the air is already pre-cleaned and the atmosphere inside consists of dust particles which are still below the 1 micron range, we are working on a pre-filter for use in conjunction with high efficiency filters, which would be of the superfine class fiber type.

SRP PANELIST: Our tests of Demisters, particularly filters and activated carbon units, in which we entrained moisture in the system, we found that the entrained water was saturated with iodine. So it is a matter of what that concentration is and what the entrained moisture is in your system. In some of our simulated tests, none of the iodine got beyond the Demister where we had a clogged condition.

QUESTION: In some of the other installations, the pads of Corning 115 glass fiber have been used for hot streams, where high reliability is required and more life is desired. I understand now that Corning is going to discontinue the manufacture of that fiber. That being the case, I wonder if there is a suitable substitute available for that service?

COMMENT: Owens Corning furnished some pre-filters made with 115-K which we used fairly widely in the program, and the fiber they have proposed to substitute for this, which is 135-C. The 135-C is rather a straight fiber. The

filters were placed in a single bank at Hanford for 18 months or so, and the dust removal was about 75 to 50, the lower ratio being the 135-C. Corning would like to discontinue the operation of the machine making the 115-K, because it is run only two weeks out of the year. Notwithstanding these results, they would like us to forecast needs and try to find a substitute, so that they can set about doing this. They do not have a definite date, and they will not give us a date for shutting off that fiber manufacture.

QUESTION: I would like to ask Mr. Megaw in connection with his iodine releases in containment vessels, whether he has attempted to fractionate the particulate activity in the air to determine to which fraction of the particles the iodine seems to be sticking?

UKAEA PANELIST: The answer is yes. The first series, the ones we did in the containment vessels, was in May 1960. On this we measured the fraction of the iodine which would attach to particles, and we tried to get some idea of the size distribution. But it didn't work out very well. In fact, we didn't get any results that we could believe in, due to the fact that it was the first time we had done this and the equipment wasn't all that it might have been. But the results indicated that the disappearance of the particulate iodine from the atmosphere was a little bit faster than that of the nuclei in the atmosphere. I do not really know what this means. It could mean they are either smaller or larger. Certainly, the results could have been explained. We felt the results could have been explained if the iodine was attached to particles in the 1, 2 and 3 micron range.

We recently had the opportunity of doing several other runs last May. We had a sampling line which contained a four stage cascade impactor, plus a filter which we thought would take out most things above 1 micron. We then followed this with a diffusion battery on which we got some idea of the size of the inactive nuclei in the atmosphere; that is, the ones that didn't have iodine attached to them, and also the particles which did have iodine attached to them. These results came out fairly consistently, from memory, at 0.05 micron diameter.

PANEL CHAIRMAN: I am going to take advantage of the chairman's position here and say that unless some real stimulating question comes forth, we will consider that the panel has discharged its obligation right now. We are indebted to the panel members here, both from the U. K. and the U. S., for providing a very stimulating discussion.

PANEL D - ROUND TABLE SESSION
Thursday Afternoon, 24 October 1963

MISCELLANEOUS PROBLEMS TO BE CONSIDERED

a. Fume Hood
b. Plastic Materials
c. White Rooms
d. Particle Size
e. Rapid Determination of Activity
f. Iodine Sampling
g. Incineration

PANEL CHAIRMAN: Good morning, gentlemen. Panel D, Miscellaneous Problems To Be Considered, has the following panel members:

R. Dennis, HACL, Chairman
M. W. First, HACL
D. P. O'Neil, ANL
L. Gemmell, BNL
R. E. Adams, ORNL

You have probably observed in our list of subjects for this last panel we have material which cannot be attributed to any one category. We have had one paper on special type of materials used in fume hoods, but that subject has not come up to any great consequence in any of our discussions so far. Plastic materials have been touched on and we intend to cover that somewhat during our discussion. White rooms merit comment. Particle size has been treated to a great extent in many of our discussions. In rapid determination of activity we have had several general methods indicated, and our BNL Panelist has mentioned he would like to treat the subject specifically from the point of view of iodine monitoring. In view of the very enthusiastic response and interest in problems relating to iodine this is a worthwhile part of our agenda. If time permits, I would myself like to take the liberty of making a few comments on incineration problems.

The only instructions to the panel members is that we are pressed for time. If we can hold our comments down, as individuals, each to five to seven minutes, it would be quite helpful. At this time I would like to turn the discussion over to the BNL Panelist.

BNL PANELIST: My only reason for being on the program, I presume, is that we depend very heavily on monitoring. We have no ultra-filters in our reactor setup, and anything that goes along, we have to depend strictly on our monitoring methods.

The reactor* itself is a cube of graphite in which we move approximately 270,000 cfm of air through it for cooling. There are approximately 615 horizontal holes and about 4,900 highly enriched fuel elements. The flow comes up to the

*See "The Use of Activated Charcoal Iodine Monitors During and Following a Release of Fission Product Iodines," by Charles F. Foelix and L. Gemmell, p. 629.

center gap and flows bilaterally. It is monitored by moving tape particulate monitors. Inlet air filters are ordinary glass-wool to filter out the large "sticks and stones."

Other filters in the duct are filters that have been in there for 12 years. The filter efficiency is not high; it is only about 80%. After 12 years of gathering dust, we are looking forward to in-place filter testing very soon.

Slide No. 2 is a typical charcoal trap. The filter in the trap is normally changed at two- or three-day intervals, and the quantity of iodine-131 on the filters is determined by counting in a calibrated gamma spectrometer after sufficient time has elapsed to permit the decay of the short-lived iodides. After all necessary decay and samplings are made, the total stack release of iodine-131 is computed. The system is used primarily as an inventory monitor to measure accurately the total release of iodine-131 and it is not necessarily intended to pick up accidental releases.

During a two-day period about a year ago there were three distinct short-term releases of activity which were the result of a partial cladding failure in one of the fuel elements. Although these releases were not of serious proportions, they did provide valuable experience in the area of reactor effluent monitoring under emergency conditions. The three releases were characterized as follows.

On the operational monitors the north duct particulate monitor showed a peak and the presumption is that some material in the cooling air had been activated. The classic example is that a vehicle, an automobile or a stationary engine, operating near the air intake with the exhaust fumes being sucked into the reactor. When only one duct monitor shows a peak, it indicates that the activity has been released in that half of the reactor.

The Kanne chamber monitor responded promptly to full-scale reading and returned to a normal, which indicated a short, concentrated release of gaseous activity. The particulate monitor in the base of the stack showed a peak of modest proportions which would not in itself be cause for alarm.

The first release occurred at about 2100 hours on September 10, 1962. The next morning the charcoal trap was taken from the sample line and placed in the gamma spectrometer within ten minutes after removal. It was immediately apparent from the display of the gamma spectrum on the scope of the analyzer that a release of fission product iodines had occurred. The 0.365 mev photoelectric peak of iodine-131 was prominent, and almost as large as the 0.53 mev peak of iodine-133. Normally the iodine-131 peak is almost entirely masked out, and much smaller than the iodine-133 peak. Under normal conditions the reactor will release about 8 mc of iodine-131 a day. It was calculated that the first release totalled something in the neighborhood of 70 mc. The second and third releases contained 180 and 300 mc of iodine-131 respectively.

The problem of locating the fuel element is one of the things that would be of interest to you.

Needless to say a great deal of effort was being expended to find the source of the releases, but finding one or two damaged fuel elements out of 4,900 elements is a difficult and time-consuming task. Between the second and third release the reactor was shut down, and the elements in several suspected channels were inspected. These channels were suspected because of higher temperature readings on the thermocouples in the channels. The elements appeared normal, so the reactor was brought up to reduced power level, and shortly thereafter the third release occurred.

It was decided to reduce power level and take air samples from each fuel channel. The samples were taken to the charcoal traps and the traps were monitored with a survey instrument. One fuel element was found to be much higher than the rest. The reactor was shut down and elements in the channel were removed and inspected. Two of the four elements appeared to be damaged. One had a spotted appearance, and the other was badly blistered. Subsequent hot-cell inspection and testing of the elements proved that they were the source of release.

The reactor was again brought up to power. No further distinct releases occurred, but the stack samples showed that the iodine-131 being released was much higher than normal, initially about ten times higher and slowly tapering off to normal in about 20 days. It was theorized that some of the iodine from the elements had condensed or adsorbed on the duct work and exit air filters and was slowly subliming or exchanging out into the air stream. The analysis of the additional air samples taken on charcoal at the north and south duct monitors and at the base of the stack, supported this theory.

On September 15, about three days after the last release, a set of 24-hour samples were started, with samples taken at the north and south duct monitor locations, and at the stack. When these samples were analyzed, the following information was obtained: The ratio of iodine-133 to iodine-131 in the south duct was 8. The north duct was 1.4. And in the stack, 1.2.

The rate of release and the ratio found in the south duct was normal. The ratio in the north duct and the stack samples was not as low as one would expect for three- or four-day old equilibrium fission products. However, if the sample results were corrected by subtracting the normal amount of iodine-131 and iodine-133, the remaining activity shows a ratio of about 0.1, which corresponds to several-day old equilibrium fission products. Also, it was found that the amount of iodine-131 being released from the stack was almost twice the amount found in the duct samples, indicating that about 50% of the iodine-131 being released was coming off the filters.

These samples, then, supported the theory that the iodine released from the fuel elements had adsorbed or confined on the duct work and filters and was slowly subliming or exchanging off into the air stream. Subsequent experiments showed that the adsorption-exchange mechanism was the dominant one. In the course of trying to develop a new operational iodine monitor, stable iodine-127 was released into the reactor to produce iodine-128 to check the response of the new monitor. Each time this was done the stack charcoal iodine monitor showed a sufficient increase in the amount of iodine-131 present. The fact that iodine-127 is put in, and iodine-131 comes out indicates that even under normal operating conditions there is iodine adsorbed in the reactor structure which exchanges off into the air stream. This knowledge of the behavior of iodine suggests a method of decontaminating reactors or other structures.

The system of iodine monitoring in use at BNL has proved to be a satisfactory means of measuring the release of iodine-131 to the environment under emergency conditions. Although the cooling air effluent is monitored by other means, which indicated that something had been released, only the system using activated charcoal traps and gamma spectrum analysis could reliably indicate that a release of equilibrium fission product iodine had occurred and accurately measure the amount released. Other media tested by BNL and others do not have as consistently high collection of fission release as the charcoal traps. The system was also used for locating the defective fuel elements, and explaining the behavior of iodine within the reactor structure.

ANL PANELIST: What I would like to discuss is the change in philosophy in some areas of Argonne National Laboratory that have taken place as a result of corrosion problems we have run into in our analytical problems; we have introduced about 200 plastic hoods in the last two years. This was necessary because the chemists encountered corrosion products in some analyses in spite of using plastic sheeting, tape, and other coatings.

In some of the other hoods in which we have used approximately 6,000 liters of concentrated acid during the past ten years, we are still working with the same hoods. These hoods, which were cleaned regularly, were not satisfactory for the chemists. A testing program was started in 1959 for corrosion, heat resistance, decontaminability and fire resistance. Some of the materials tested were Hetron 92-55, a polyester with 5% antimony trichloride, 5% styrene laminate, a fiberglass reinforced material. PVC, vinyl, and some others were also tested.

As a result of these early investigations there were some heat tests; materials were placed on top of heating elements. The decontamination tests consisted of putting a Pu solution in various concentrations on these materials, and then cleaning them off. One decontamination experience which tended to direct them toward Hetron-92 was the fact that we had a Hetron hood from some previous work and the Pu was readily removed. Some of the other plastics in that hood could not be cleaned. As a result some large hoods were fabricated, 26" deep, 42" wide and 42" high. In-place fire tests were run on the hoods and the ducts, which were fabricated of the same material. A typical laboratory hood loading was used: Four $\frac{1}{2}$-gallon cardboard containers; two 1-quart cardboard containers; 18 empty 250 PV wash bottles; 48 sheets of Kleenex; twelve feet of $\frac{1}{2}$" inside diameter rubber tube; 10 feet of 20 mm polyvinyl sheet; 10 feet of absorbent paper; 1 pound of acetone; and some other items. I choose to think this was a collection of materials from various hoods, and that no one hood ever contained so much rubbish!

Five tests were performed. On the first test, using the Hetron 92-55 hood, in starting a fire in the acetone, the prefilter plugged at 37 seconds, and 30 or 40 people were driven from the building. The same test was run with a stainless hood, and it plugged in 100 seconds, or in about three times it took with the Hetron 92.

It was promptly concluded that whatever hood is used, a fire resulting in the plugging of a prefilter would result in the contamination of the laboratory. It was determined that a sealed extinguishing system was needed; it was tested in the stainless and worked. Tested in the Hetron hood, it worked equally well. When the extinguisher was activated at 11 seconds, in 60 seconds it was too far gone, and it was plugging.

The fire-extinguishing system then became an integral part of the hood. It consisted of a cylinder of CO_2, with discharge head, which could be manually or automatically actuated; a sealed nozzle to protect it from corrosion; two outlets in the hood; four in a double box, one in a glove box and two in a large hood; a heat detector located in the top rear of the hood; a switch that will actuate an alarm in the hood and cause an alarm to indicate at the fire board so that the fireman can respond on a single corridor, checking the laboratory for the noise and red button.

The hood costs about $1,000 for an order of 127. The stand on which the hood is placed, including the rheostats, the service controls, and electrical outlets costs about $156. The fire-extinguishing system for the 93 installed hoods costs $300, for a total of $1,456 for one hood.

The hoods are easily decontaminated. They have a safety glass front at 7-½°. The chemists seem more than satisfied. One small fire has left a small spot in one hood. A considerable amount of acid has been used without any detectable damage to the hood. Where the fire-extinguishing systems are to be used in our hot chemistry laboratories these hoods will be employed. There are none now being used in the wet chemistry laboratories. That concludes my comments.

HACL PANELIST: I would like to extend the remarks of the ANL Panelist about plastic structures. This came to mind from Mr. Boise's talk when he described a PVC blower which failed in a matter of a few weeks. This is perhaps unusual for this type of equipment, because plastic materials are coming into widespread use industrially and in general their service and performance is excellent.

I am familiar with one PVC installation which is working on fumes from aqua regia pots dissolving platinum metals which has been in operation for approximately ten years. I am familiar with many glass-reinforced polyester installations in pickling and etching service of a very severe nature, including hydrogen chloride, oxides of nitrogen, and hydroflouric acid.

One of the principal problems with rigid PVC construction is its inability to withstand vibration, and I wonder whether the blower which was described by Mr. Boise might not have become out of balance because of the deposition of particulate matter on the blades.

There are many advantages to the use of plastic materials. One is the specific gravity of glass-reinforced polyester, about 1.5, as compared to a specific gravity of 8 for steel, or perhaps 9 or more for alloy steels, which the plastic structure would be replacing. Even though the wall thickness for a plastic structure would tend to be larger, perhaps 3/4" wall thickness for a plastic structure as against 1/4" for a steel or alloy structure, there is still a net gain of about 1/2 in weight for the plastic structure. This, of course, makes it a lot easier to handle; smaller supports are required.

I have in mind one structure constructed of glass-reinforced polyester which was supposed to be the largest plastic structure ever built. It was a scrubbing tower of 10-1/2 feet in diameter and some 56 feet in height. It was shipped on a flatcar and erected in place with a crane; the structure is self-supporting. The point I am trying to make is that these materials are very useful and are solving many problems.

I was recently at a factory making this type of structure, and I was surprised to see they were making axial flow blowers of quite large size; perhaps four or five feet in diameter of glass-reinforced polyester; this is a real achievement in terms of strength and durability because the speeds of this type of centrifugal machines tend to become quite high.

Turning to the use of fibers, I think you have already heard something about the use of teflon fibers in the Demisters which have been discussed the last few days; of somewhat older service are saran fibers which we investigated at the Harvard Air Cleaning Laboratory back in 1950, and which have been in service for corrosive conditions in many installations since then. I believe there is an installation here at Oak Ridge that was installed within the last couple of years and is in continuous service.

In addition to saran, orlon and Dynel fibers are available for deep-bed filtration and for gas adsorption in scrubbing; all of these fibers have been woven into cloth or felted for use with industrial, cleanable bag filters.

One limitation of plastic materials is temperature. Polyester materials, polypropylene materials now are available with a temperature limit of 350°F. Teflon, of course, will go up to 450 or 500°F, at least for some limited period. Each year, when the reviews of plastic materials appear, the temperature limit seems to go up ten, or perhaps twenty degrees; we are continually finding improved materials of a plastic nature.

I would like to now turn to the White Room topic. A new Technical Order of the Air Force has recently appeared; it is a revision of Work Standards for Clean Rooms and Clean Benches. There is quite a voluminous report and I certainly have no time to review it in anything like its entirety. I would like to bring up the matter of a leak test or an in-place test for the filters for clean work stations, as they are called in this technical order. The test consists of a cigarette smoke aerosol generated by a hand-squeeze bulb from an ordinary cigarette; the detection mechanism is listed as some type of a smoke photometer. The smoke photometer probe is followed around the edges of the filter and across the face; if there is any leakage, the filter is improperly installed. No standard is given as to what constitutes a leak; that is to say, no percentage increase or ratio. We are pretty much left with the same sort of thing we were discussing this morning; namely, there are no standards except each person will have to decide himself what his particular standard is.

The thing in the TO that puzzles me is that if the group assembled here, with all the experience and know-how on absolute filters, cannot agree on what a proper in-place standard test would be, where are we going to go to find this information? Who are we going to ask? Most of the know-how is sitting right here in the room. I would like to suggest, as a final word, that perhaps we are talking about two different kinds of tests. On the one hand, the standard DOP test which is made by the manufacturer is an index of performance. It has no meaning in the sense that it corresponds to any actual situation in practice. I don't know anyone who is interested in filtering 0.3 micron DOP smoke in any plant in the AEC, or otherwise, and yet this test, or any other of the tests that have been suggested, provide a very handy index of filter quality.

Another type of test is the one made on site for the particular conditions that exist at an installation. This type of test can be focused on the particle sizes, the temperatures, the loadings, and so on, that apply specifically to that situation, and this is quite a different test, in my opinion, and I think the two should not be thought of as a single test. We need the specific tests for a specific installation. But I think we also need a test which gives us an index of performance to evaluate quality.

PANEL CHAIRMAN: To continue our discussion, I would like the ORNL Panelist to discuss the uncovered points in the iodine area.

ORNL PANELIST: To point up the iodine problem, it is interesting to note that approximately 50% of the papers presented at this meeting are concerned in varying degrees with iodine. This is to be compared with approximately 20% at the 7th Air Cleaning Conference. It appears that the iodine problem is larger than some had originally proposed. Over the past several years many methods have been studied and applied to the problem of iodine removal. We have scrubbers, high-temperature silver reactors, the low-temperature or room-temperature silver reactors, porous-solid materials such as silica gel, molecular sieves, and activated charcoal to name but a few.

Then we have the special methods of development, including the foam and diffusion board studies. I believe we will agree that at the present time the activated charcoal seems to be the best available solution to the iodine-control problem.

One field that we are just getting into now is the question of the other forms of iodine. This appears to be a different matter altogether. We can control, I feel, the molecular iodine releases. The other forms of iodine are different matters. The magnitude of this problem is still being defined. The lowered efficiency of charcoal for iodine present in off-gases from chemical processing plants is an excellent example. Additional studies appear to be necessary to identify and characterize the behavior of these other forms of iodine.

Another area deserving more attention is this area of in-place testing of iodine-control systems. What we need in the iodine field is a testing procedure such as the in-place DOP tests, but I probably shouldn't say that now, considering some of the remarks made here!

The use of freon adsorption in testing full-scale charcoal units is under study at Savannah River. Our English colleagues have tested the behavior of gas-cleaning systems, using iodine released within reactor containment vessels. In-place testing procedures are being studied at ORNL in support of the Nuclear Ship SAVANNAH project.

Those of us in the iodine field are still looking for a quick, simple test method. If any of you can suggest such a method, we would be most happy to receive it. That concludes my remarks.

PANEL CHAIRMAN: Taking advantage of my position as Chairman, I would like to make a few statements on the general subject under consideration. The first point I would like to cover is our impression and that of the AEC, particularly the Division of Reactor Development, with regard to a recent publication by the Bureau of Mines. The publication came into print after the 7th Air Cleaning Conference.

The Bureau of Mines has issued a publication ROI-6083 which suggests that there is available a working incinerator for the disposal of low-level radioactive wastes. My first reaction was that perhaps we had fallen by the wayside and had missed some more recent experimental data, but upon examining the report we found out that the Bureau of Mines had come to perhaps an entirely different impression of the serviceability equipment in contrast to the opinions that we had expressed at the previous AEC seminars.

We did receive communications from the field asking what our impression of the unit was, and until we received the final Bureau of Mines publications we could not answer. We discovered that the information presented was precisely what had been presented before, that no new experimental work had been done, and our conclusions of 1957, were that there was considerable experimental work to do before considering the BOMAEC 30 unit a functioning incinerator. One of the big problems at that time was the design of a gas-cleaning system which would handle any type of effluent produced with the typical laboratory-type wastes that one expects to burn.

Many of you recall we had unsuccessful experiences with the BOMAEC 30, perhaps partially due to modifications. The net result was that we could not produce an effluent that could be filtered with glass fiber bags, and evolving from that was the incineration package which Mr. Bloore discussed earlier.

As far as the Nuclear Defense Laboratory unit is concerned, I think that Mr. Bloore covered this quite well. We want to emphasize again that it is strictly an experimental unit, and there is considerable flexibility in the design of this package to permit a variation in the mode of admitting air, both as to quantity, velocity and direction.

We accept criticisms on the present grate design. We expect the Navy to follow through with some burning material, and in the experimental unit intend to use lightweight, cheap metal gratings of dimensions sufficient to prevent the actual fall-through of material, that is, a fixed grate. But we could see no point, in the experimental unit, of putting in any involved, rotating device which would require elaborate mechanical shields. What has been presented as a grate design we recognize as only a temporary device until we have time to iron out some of the burning features, and the same applies to the charging hopper which is also subject to many criticisms. This was just one simple way of getting the material into the unit while the fire was under way, then permitting drying.

A loading fork is used, and the sensible question has been put, "What if you forget and leave the loading fork?" I dare say, if you were loading it with stainless, it would melt in no time at all! During the experimental work we know that Mr. Bloore will be alert and see that the fork doesn't stay in the fire!

Our experience was, in the laboratory studies, that one minute would be required to take a charge containing 90% moisture; one minute's exposure to a hot gas had dried it sufficiently so it could be dumped on the grate without interfering with the burning.

I have had a chance to discuss incineration problems with a few people here. One question was brought up as to the matter of charging intermittently; our philosophy has been that if an incinerator is charged intermittently we would experience the whole process of heating up through any refractory materials. During this period combustion products will be high inorganics, and constitute rather a difficult filtering load.

The matter of using fabric bags has again been brought up. We did find that glass fiber bags did do a fair filtration job with the BOMAEC unit, provided that the unit was burning properly, but whenever difficulties were encountered there was a strong tendency to plug the fabric, and consequently a rapid rise in resistance; because of the nature of the fabrics themselves and exposure to acid materials there was a tendency to rupture along the seams.

Plastic media, or vegetable fibers, do not seem to be a safe way of handling filtering problems when considering the fire problems. Even if water sprays are used to precool the stream, the eventuality must be faced that water power may be lost and the filter bags burned.

As far as the wet collection is concerned, it sounds easy, and if the facility is to handle the liquid-waste problem, a wet-collecting system might handle the effluent. However, there has been a rather sad experience in some locations in the field due to corrosion.

The net result? I do not think there is any incinerator yet on the market which will solve anyone's waste-disposal problems. I hope, as a result of Mr. Bloore's test at Edgewood, that we can eventually come up with something more fruitful.

With regard to iodine, which has been sort of a pet peeve for several years, I would like to mention that with carbon tetrachloride and iodine-131, salted with iodine-137, we did find in the high concentration range that there was no effect whatsoever in the efficiency of Columbia-type activated charcoal. However, it is not a realistic type of test aerosol, and we certainly would prefer to use some other type of material. You might even feel that the carbon tetrachloride might be an adjunctive selection. In this case, it is a pretty

darn good solvent for iodine.

A few questions might be posed on the degree of fixing iodine to charcoal beds; what type of creep might or might not be expected; what type of interchange we might get if we are testing iodine filters with normal iodine, with loadings of many orders of magnitude higher than anything we would expect as a radioactive form? We are also interested in the N. S. SAVANNAH program.

COMMENT: We got into the instrument problem because of the problem of the N. S. SAVANNAH being a mobile reactor, and the requirement to test the filters for iodine retention. We cannot depend upon the use of iodine-131 when we are away from certain places in the United States; there is no source of iodine-128 aboard the ship, and we use 25-minute life material when we are away from the coast. It takes several weeks to get activation analysis results from iodine-127 tests. We had to have a ship-board means for testing the iodine penetration of the filters.

Contrary to the situation at National Laboratories or shore-based plants, we cannot afford to keep a chemist aboard the ship for wet chemistry work and we have the problem of training a technician to interpret a routine chemical analytical technique.

Having checked various methods we selected the electrical conductivity meter, which is based on a small flow of potassium iodide solution over an electrode. The sample air stream also passes over the electrode and the difference in conductivity is read on a recorder. A strip-chart record is available. If we all work on a simple system perhaps we can develop some way to cut our filter checking costs. We will have a report published on this very soon.

UKAEA COMMENT: I thought that following Mr. Dorman's description of the method, that you might like to see photographs we have taken, in order to analyze the effect of particle size and penetration. The left-hand side of Figure 1 is of the unfiltered cloud of sodium chloride, and that on the right-hand side is of the same cloud after passing through a 99% efficient filter, a fiberglass filter, of the aerosol type; and this seems to show not only an absence of the larger particles, but a pronounced absence of the smallest particles, as well. Figure 2 shows the sheet with three curves obtained at three different velocities in the filter, indicating very clearly the maximum particle size penetration and the peak penetrations in each case. As I said earlier they are two or three times the average penetration for the whole cloud.

As a matter of further interest, this is based on the use of sodium chloride which is a cubic material we are hoping to extend to plutonium, which is also cubic, and because of the fact the shape factor is eliminated we would expect that we could show an effect directly due to the density variable.

PANEL CHAIRMAN: Is there any significance to the apparent shift toward a slightly smaller diameter with the velocity, or is that the way the curves came out?

UKAEA COMMENT: We think there is just a technical effect due to velocity which gives a shift to a smaller particle size.

PANEL CHAIRMAN: Thank you. Having already gotten out of sequence, I would like to try to get into sequence again and entertain comments or questions from the floor directed toward Mr. O'Neil, and possibly Dr. First, as a bit of overlap in the experience, there; and as far as fume hoods and plastic materials are concerned, I think you can direct your questions either way.

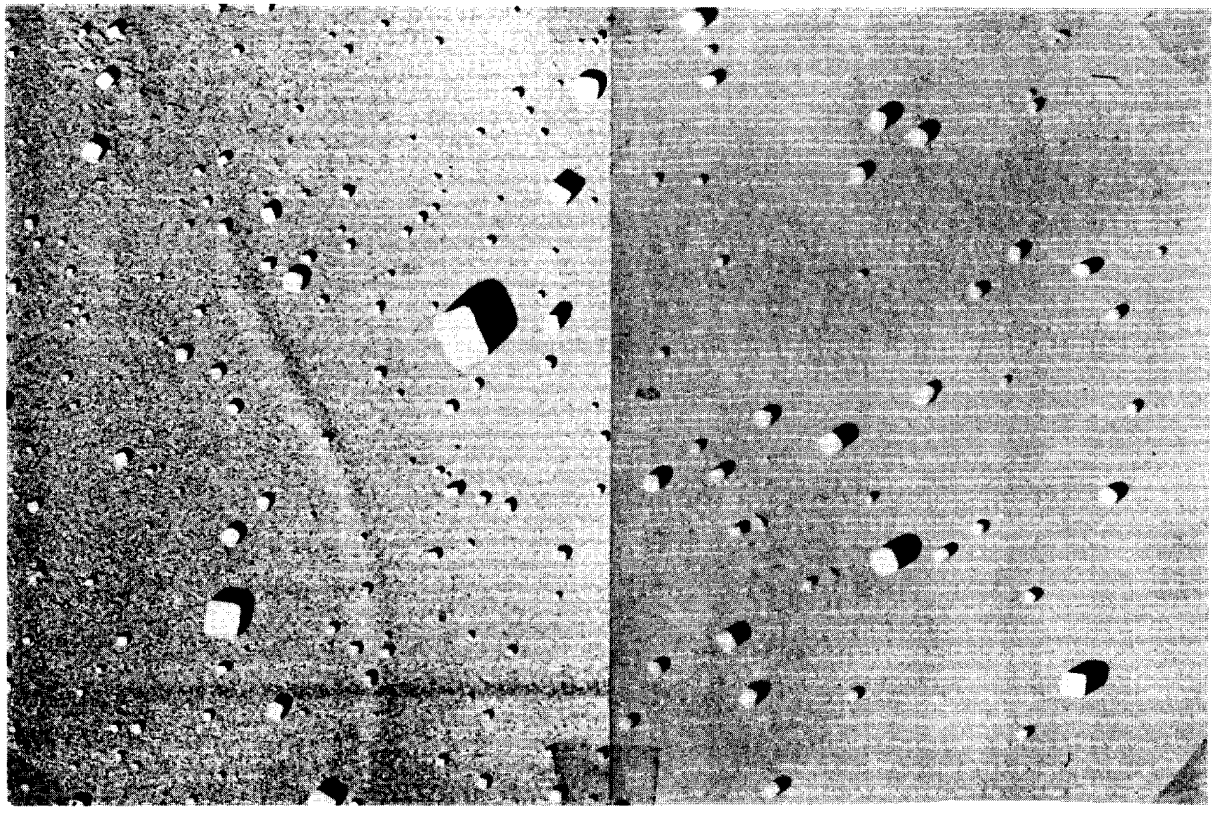

FIG 1 ELECTRON MICROGRAPHS OF SODIUM CHLORIDE

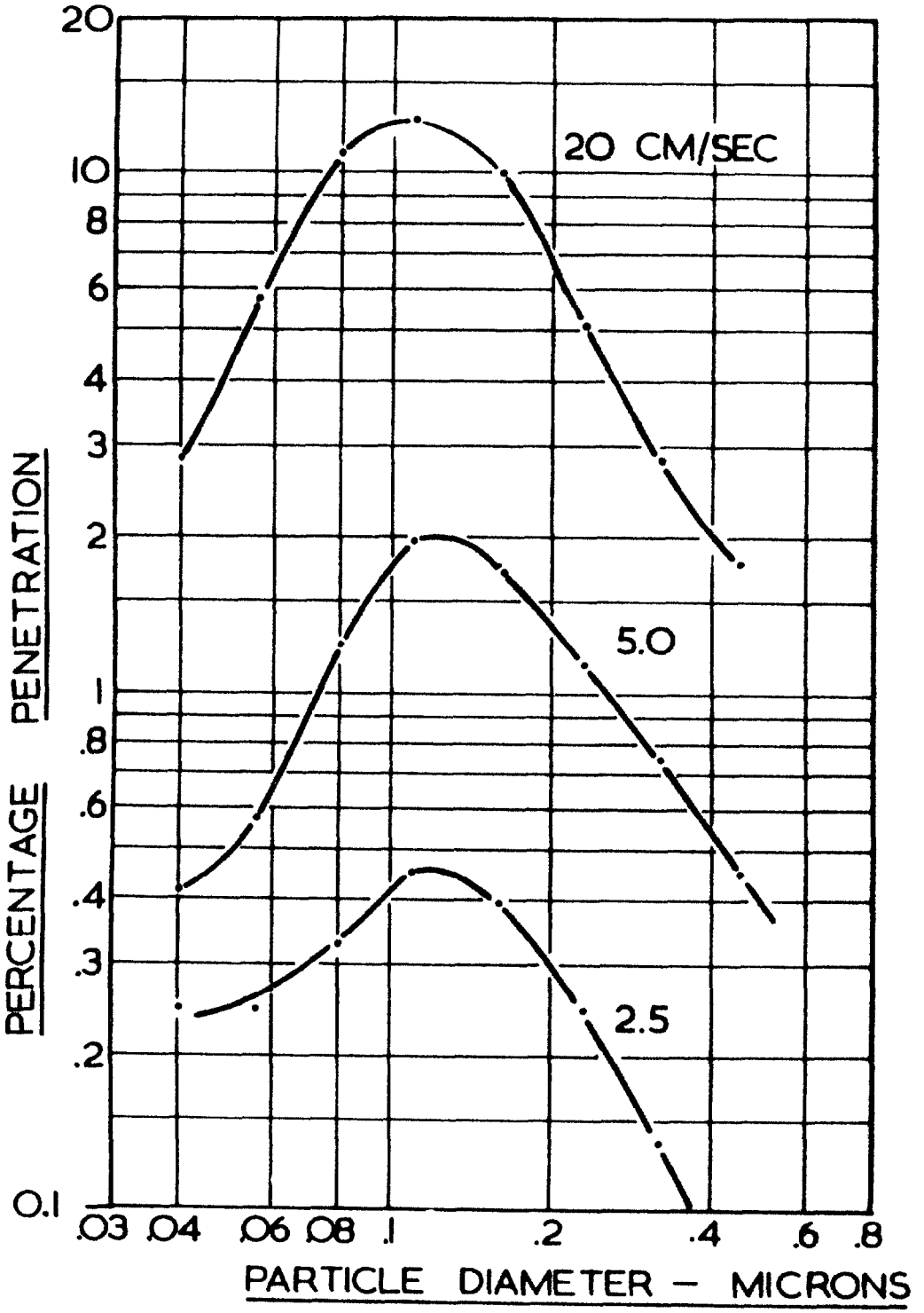

FIG 2. PENETRATION v PARTICLE SIZE FOR SODIUM CHLORIDE.

NLO COMMENT: I would like to clear up the HACL Panelist's assumptions, and perhaps apologize to him for misleading him in the assumption that we did not like PVC. Actually, we have carried on quite an expensive program in testing plastics, and we find that PVC is a very good material. We are in the process of doing some design work for hoods in PVC, and with duct work. The scrubber blowers were PVC. They were direct-driven, and were furnished as an integral part of the scrubbers. We found, on examination, that there was very little erosion but no deposition. Erosion of the PVC itself was a mis-engineered proposition due to the fact that the manufacturer used a greater peripheral speed than the PVC could stand, and they actually flew apart.

I would like to also ask the ANL Panelist if he has any data regarding the concentrations of the acids, and the types of acids that he used in Hetron polyester hoods, and if he knows what the face velocity of air intakes were across your hood faces?

ANL PANELIST: About 135 foot per minute. I do not know the acid concentrations.

The tests that we ran on the materials, but not in a fabricated hood, included immersing these sheets in concentrated HF, HCl, nitric, and sulfuric for periods up to months both concentrated and dilute. It consisted of exposing panels in hoods where this type of work was going on for months. We did have one early hood (or glove box) used for all our Pu operations, before Pu became so common at the Laboratory. Acids were used extensively. I cannot tell you how much, what kinds, or when, but I know they were used with no problem of corrosion or deterioration of the surface.

NLO COMMENT: We had some tests run on several similar plastics and rather than running them immersed in the concentrated acid solution, we suspended them above a flue system in heated vapors at about 100° up to 150° and found quite a bit of degradation with the percholoric, and also with concentrated nitric acid.

PANEL CHAIRMAN: After all that acid left the multiple-eductor units on the roof, did you experience any difficulty with automobile paint or painted structures? It appeared to me there was a little bit of rust on some of the assemblies on the roof that was intimately associated with the eductor unit itself.

NLO COMMENT: The particular tests that we ran were closed. In our general operations we had no concentrations up to that amount. The rusted units you saw were the results of a concentrated nitric acid atmosphere from another stack. These, incidentally, are on the roof of the building approximately 50 feet high.

QUESTION: Relative to the use of plastic materials, the AEC design criteria makes use of the NFPA Code. How do you justify the use of plastic materials? The Code mentions metal duct work.

ANL PANELIST: We have received approval for the installation of these hoods, not only the hoods, but the lateral duct work, vertical duct work, glove box, holders right on through the entire new hot chemistry laboratory attached to the Chemistry Building.

QUESTION: Were concessions made such as having additional sprinkler protection?

ANL PANELIST: No. We do not have additional sprinkler protection there. We do have the fire protection systems integral with 93 of the 127 hoods. I know of no other concession we have made.

QUESTION: I am somewhat surprised on the use of CO_2 for two reasons. One, the great turbulence that you get in the hood, which does cause, if you have any contamination associated, a great spread of contamination will result with the use of CO_2 in the hood or particularly in the glove box. Secondly, was consideration given to use of the dry chemical compounds in preference to CO_2, because of much greater fire-kill capabilities?

ANL PANELIST: Yes, dry chemicals were considered. A number of tests were run with the CO_2, in which CO_2 was discharged through .04" diameter orifice, I believe. The pressure never went positive in the hood, and the fire was extinguished on about a half square foot surface of kerosene and alcohol in a matter of 20 some seconds.

The tests in an open-faced hood did result in small amounts of the CO_2 coming out into the room, and that is the risk that they were willing to take; the contamination of the room, as opposed to more extensive contamination if no extinguishment was used at all; or in effect, the ruination of the experiment if dry chemicals were employed with possibly plugging of the filter.

QUESTION: Might we infer from the possibility of plugging of filters that efforts at fire-resistant filters may be not as important as arranging quick detection and termination of exhaust ventilation, if we must realize we are going to plug up and contaminate our work area anyway?

ANL PANELIST: We tested the speed of response to our sensing element, and we found that 500°, it would respond within 6 seconds. At 160°, that temperature was required, I think, for a little over a minute.

QUESTION: Are any precautions taken against accumulations of perchloric acid in the plastic, duct work, or in the filters perhaps?

ANL PANELIST: We have done our utmost to discourage the use of perchloric. There is a small perchloric scrubber in use at the present time which will confine, I think it's 90%, of the perchloric to the scrubber itself.

In addition, in open-faced hoods we use a perchloric scrubber that was developed by Dr. Silverman at Harvard, or modifications, in three or four of our laboratory hoods.

We have had so far no untoward incidents as a result of the frequent replacement of duct work which we have had to do.

PANEL CHAIRMAN: I would like to thank all the panelists for improvising considerably and abbreviating their discussions. And at this time we will turn the meeting over to Mr. Belter and Dr. Silverman.